Vol. 46, No. 3 May, 1939

Psychological Review

EDITED BY
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PUBLISHED BI-MONTHLY
BY THE
AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.
PRINCE AND LEMON STS., LANCASTER, PA.
AND OHIO STATE UNIVERSITY, COLUMBUS, OHIO

Entered as second-class matter July 13, 1897, at the post-office at Lancaster, Pa., under Act of Congress of March 3, 1879

PUBLICATIONS OF

THE AMERICAN PSYCHOLOGICAL ASSOCIATION

WILLARD L. VALENTINE, *Business Manager*

PSYCHOLOGICAL REVIEW

HERBERT S. LANGFELD, *Editor*
Princeton University

Contains original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 540 pages.
Subscription: $5.50 (Foreign, $5.75). Single copies, $1.00.

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COMBINATION RATES

Review and Bulletin: $11.00 (Foreign, $11.50).
Review and J. Exp. (2 vols.): $17.00 (Foreign, $17.75).
Bulletin and J. Exp. (2 vols.): $18.50 (Foreign, $19.25).
Review, Bulletin, and J. Exp. (2 vols.): $23.00 (Foreign, $24.00).

Subscriptions, orders, and business communications should be sent to

THE AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

THE OHIO STATE UNIVERSITY, COLUMBUS, OHIO

VOL. 46, No. 3 MAY, 1939

THE PSYCHOLOGICAL REVIEW

THE CONCEPT OF DIRECTIONAL DISPOSITIONS

BY F. A. KINGSBURY AND H. A. CARR

The University of Chicago

Previous articles [1, 2] have pointed out that systematic psychology is finding itself compelled to recognize and make room for new categories and concepts which have pushed their way in from the younger branches of psychology—differential, applied, social, genetic, abnormal; that most of these categories have been used to refer to the organized individual and only by implication to mental activities; and that they need, therefore, to be critically examined with respect to their psychological implications and systematic significance. Such an examination should relate these categories to the standard types of mental activity; it should evaluate them as orienting viewpoints in psychology; and it should inquire what unanalyzed assumptions must be clarified before they can attain full respectability as major rubrics in systematic psychology. It was further pointed out that in the psychology of the future the categories of 'abilities' and 'traits' promise to be significant ways of regarding not only particular individuals, but human nature and activity in general.

It is the purpose of this paper to examine a third class of individual characterizations. This includes a large and familiar variety of organic dispositions, distinguished by definite directional reference, and commonly designated by such class-names as attitudes, opinions, beliefs, sentiments,

[1] H. A. Carr & F. A. Kingsbury, The concept of ability, PSYCHOL. REV., 1938, 45, pp. 354–376.

[2] H. A. Carr & F. A. Kingsbury, The concept of traits, PSYCHOL. REV., 1938, 45, pp. 497–524.

prejudices, likes and dislikes, desires and aversions, interests, purposes, wishes, appetites, wants, habits, drives, and many other terms, all of which we shall embrace, for convenience, under the collective term, 'directional dispositions.'

The common nature and psychological significance of these varieties of directional disposition will first be summarized in a series of propositions which will then be considered in order, followed by a brief examination of other characteristic features.

I

A. Every individual reacts selectively in respect to present objects and prospective goals, and this fact must be adequately explained by any psychology that professes to be systematic.

B. This selectivity is not a simple function of the objects, but must be explained in part in terms of the constitutional conditions of the organism, as they relate to those objects; meaning by 'constitutional' both innate and acquired conditions.

C. These constitutional conditions not being directly observable, their existence and nature are inferred from observations of the individual's behavior; hence, they are conceptual in nature.

D. Because unobservable, these constitutional dispositions must be identified and named in terms of the goals or objects to which they refer, hence the characterization 'directional.'

E. Directional dispositions, being (by definition) relatively permanent and ingrained, can therefore serve as ways of describing and explaining the reactive nature of the individual on the basis of his observed conduct, and of predicting his probable future behavior in respect to goals.

Let us consider these propositions in somewhat further detail.

A. The Selective Character of Mental Activity

A primary fact about mental activity is that it is always discriminative and selective with respect to its stimuli, its

mode of response, and especially its objects and goals. When we call behavior adaptive we imply that the organism reacts differently toward those things which have positive value for it, either as means or ends, than toward those whose value is negative or indifferent. This selectivity we emphasize by the use of such terms as attention, discrimination, differentiation, abstraction, elimination, motivation, choice, adaptation, and the like. The adaptive response may be of any degree of completeness; it may be merely a simple, perceptual or ideational apprehension of the object; or it may involve evaluation of the object; or it may operate to change in some way the object or the relation between it and the organism, and thus be properly characterized as goal-seeking. In any case, it is selectively directional in nature.

It is important to note that reaction *in reference to* an object must be clearly distinguished from reaction *to* a sensory stimulus. The latter may be no more than the occasion or precipitator of the response. What is of importance is the object, present or prospective, in reference to which the individual acts. Such an object is no simple, indifferent, physical entity; it is always a meaningful complex, a *psychological* object; and the nature of his reaction to it is determined by its meaning for him.

The variety of psychological objects in reference to which an individual may react is unlimited, and his modes of reaction toward them, as we shall see later, hardly less so. The objects of one's thoughts, feelings, and actions, of his attitudes, opinions, sentiments, and desires, may be objects as diverse as the weather today, tomorrow, or last week; music or the Minotaur; pure truth or a defective dental job.

In general, psychological objects may be either sensory or ideational, existent or non-existent; they may be objects of thought or feeling, or anticipated goals of action; they may be concrete or abstract, individual or universal, past, present, or future, memorial, imaginary, or conceptual; they may refer to things, places, persons, groups, institutions, acts, values, qualities, quantities, relations, propositions, eras, events—to anything, in short, about which one can think, or upon which

or in reference to which one can act. How one can act in reference to an object which has no spatial or present temporal existence, such as 'equality' or 'the origin of the solar system,' is a pseudo-logical difficulty which vanishes as soon as one remembers that the object of an act is always present in goal-directed behavior, by symbolic proxy if not sensorially. Why the organism reacts in the way that it does and to the things that it does constitutes a major psychological problem. Any psychology which aims or professes to be systematic must undertake to offer a comprehensive analysis of this fact of organic selectivity. Most recent psychology, in our opinion, has been negligent in this respect, as we shall endeavor to show.

The fact of selective functioning suggests two questions. The first is, "Why does the organism react as it does to one object and not to another?" This must be answered in terms of the principles of individual inheritance and learning. The second is, "Why does the organism react now in one way, now in a very different way, to the same object?" To answer this question requires that we shall take account of the temporal variations among the conditions which determine activity.

This second question has not been dealt with so directly in textbooks of the past two decades as in earlier texts (Pillsbury's, for example), which dealt systematically with the problem of selection in terms of 'attention,' and attempted to analyze the conditions determining the direction of attention at any moment. Largely due to the behavioristic reaction against subjective and telic concepts (particularly purpose and attention as determiners of action), to its emphasis on stimulus, response, and conditioning, and to the simultaneous decline of existential psychology, attention began to disappear as a major category from the textbooks after about 1915. In consequence the importance of selectivity, as well as the necessity of explaining it, was generally lost sight of. Dynamic and motivational types of psychology have recognized and dealt with the problem of selectivity to some extent, but almost entirely as a function of certain constitutional condi-

tions; the temporal variations of direction within these constitutionally defined limits have generally been mentioned only incidentally, if at all. In spite of this general neglect, the organic conditions which determine the direction of attention and hence of action deserve much more searching systematic analysis and description than they have received in recent psychological literature.

B. Conditions Determining Direction of Selective Functioning

The direction and character of mental activity at any moment have to be explained partly in terms of the stimulating object or situation. To this class of facts the older psychology gave the name, 'objective conditions of attention.' But primarily, they are a consequence of conditions prevailing within the organism which determine the dominance of one or another of the various meanings which that object has for our present ongoing activity, and which have been called 'subjective conditions,' but which might better be called 'individual conditions.'

The objective factors, including stimulus intensity, change of intensity, extensity, change of extensity, rate of change, repetition, and perhaps others, denote conditions which are universal and relatively constant from individual to individual, so that we refer them to the character of the stimulus rather than to the individual's makeup. We must not, however, overlook the fact that, were it not for the presence of constitutional conditions within each individual to respond to such objective stimuli, they would be wholly ineffective as determinants of behavior. Their universality is often assumed to be a product of natural selection, and an evidence of their survival value; and they might better be termed 'universal' than 'objective' conditions.

Of more importance for our interest are the so-called 'individual (or subjective) conditions.' These refer to the determinative dispositions generated by the individual's past experiences, including his training, cultural background, and social pressures, but especially to those dispositions which

underlie and are relevant to his present purpose, interest, mood, and viewpoint, and which constitute corresponding modes of readiness for response. Most objects, perceptual or ideational, possess a variety of aspects, qualities, relations, functions, values, or other attributes, which have played different roles in different past experiences. The sensory, ideational, and emotional context in which any such object is experienced determines which of these attributes shall dominate and determine its meaning in that particular experience—or whether, indeed, any of these attributes shall be noticed and reacted to at all. Such recognized variations of sensory setting, viewpoint, purpose, and prevailing mood, aid us in explaining not only why different individuals react differently to a given object, but even more clearly why one individual varies widely from time to time in his manner of reacting to an identical object.

An aspect of these conditions of more importance to us than their temporal variability is the fact that repetition of these experiences and responses leaves organic traces which modify existing dispositions due to inheritance or previous learning, and which are more or less cumulative and lasting. These dispositions we call 'constitutional,' using 'constitutional' always to include both innate and acquired dispositions. It is on the basis of these relatively permanent determinants that we explain the selective aspect of behavior, describe the reactive nature of individuals, and predict their behavior in respect to particular objects and goals. Maturity and age, education and habituation, socialization and cultural determination, reflection and volition, deterioration and decline, all contribute to their establishment and breakdown.

Constitutional conditions are to be thought of as organic determiners of correlated systems and hierarchies of acts, and as constituting an integrated unity, not separate cortical structures. What we call 'a directional' disposition is any one of these systems which our interests impel us to abstract from the total, name, and treat as a unit, and which, for example, we may designate as an attitude of hostility toward beggars, a habit of analyzing orchestral effects, an interest in

numismatics, a desire for popularity or power, a drive to succeed in one's chosen profession. It is usually thought that each directional disposition possesses some kind of structural constancy within the larger integrated unity of cerebral organization, although it is not assumed that such structures must be topographically separated.

In consequence of the impossibility of direct observation of the central mechanisms in operation, and the impracticability of experiments on human subjects involving cortical extirpation or injury, most of what we know definitely about the neural mechanisms of directional response-tendencies is what has been derived from the study of animals and of pathological cases, and from genetic studies of the conditions and appearance of the various dispositions. For the most part, our description of these organic conditions must remain circumstantial and schematic.

Through numerous genetic studies we know that directional dispositions of all types belong chiefly in the category of learned behavior. Individual differences are the product of differences in cultural background, social stimulus and standards, formal training, and casual experiences, far more than of inheritance. At the same time, inherited predisposition or limitation is sometimes a contributing factor, as in appetite or lust, which are dispositions aroused by organic stimuli, but modified by experience. Because they are learned, and because they are limited to a single area of experience, directional dispositions once formed are more susceptible to modification than are abilities and traits; and the investigation of conditions of change in, and methods of controlling, attitudes, opinions, and sentiments commands increasing interest among psychologists.

C. The Conceptual Nature of Directional Dispositions

While our ideas about the physiological mechanisms of the various directional dispositions are necessarily inferential and schematic, our knowledge of the dispositions themselves is factually derived from cumulative observation of character-

istic stimuli and responses, including explicit behavior in others and implicit behavior in ourselves. Some kinds and attributes of behavior are more accurately indicative of directional dispositions than others, as, for example, verbal statements, behavior in critical and controversial situations, markedly constant or habitual types of behavior, the trend of spontaneous activities, voluntary choices, etc. Obvious behavior, because of the presence of distortions, concealments, substitutions, evasions, and the like, cannot be depended on as a sure index of attitudes, sentiments and motives. On the other hand inhibited and erroneous responses are often regarded as valuable indices of unexpressed motives.

Dispositions, of course, must be clearly distinguished from the acts through which (as common speech has it) they find their expression. Acts are observed; dispositions are inferred. Being unobservable, directional dispositions are conceptual in nature. They are intellectual constructions, by means of which we endeavor to account for the observed uniformities and patterns of organization in diverse individual conduct; and the test of their validity lies in the effectiveness with which they account for the observed phenomena.

Because these concepts have originated in everyday life, we need not expect uniformity and definiteness in classification, nomenclature, and application, such as we expect in the case of reflexes whose motor components are constant and describable, or even in the case of ability and trait concepts which have come to be operationally defined, such as the Stanford-Binet Mental Age or I.Q., or Allport's Ascendance-Submission. The fact of their subjective character helps also to account for the familiar inconsistencies in interpretation and appraisal, by different observers, of an individual's motives and dispositions, as well as of his traits.

A given directional disposition finds expression in acts which are, as a rule, too numerous and varied to be inventoried. Its responses may be either overt or implicit, literal or symbolic, preparatory or consummatory, self-adapting or object-controlling, incomplete or complete, cognitive, conative or

affective, adient or abient, adaptive or non-adaptive in effect, successful or unsuccessful. Usually any given disposition finds expression, at one time or another, in acts of most or all these types. There are a countless variety of acts through which, for example, a person's love for music finds expression—playing (various instruments, solo or ensemble), singing, whistling, humming, auditory imagery, score-reading, concert-going, buying phonograph records, conversation, choice of occupation, choice of leisure activity, individual vocabulary, facial expression, posture, foot-tapping, tone of voice, etc. A directional disposition can never be adequately pictured by a simple S–R diagram, for it implies neither the performance of any particular act, nor the operation of any particular neural arc. The variety of ways in which a given disposition will find expression, first and last, usually totals very large, because each separate response is a function not only of the object-set but of all the relevant circumstances, organic and environmental, prevailing at the moment of response.

In spite of this variety of expression, any given directional set exhibits an integrated and organized character, which leads us to conceive it as in some sense unitary. Its responses are not single, stereotyped, independent movements, but organized systems, classes, or groups of acts, varying with circumstances while still sharing a common goal or object of reference, and a common functional significance, positive or negative, toward that object. Often their organization is complex; one response is subordinate to a second, inclusive of a third, coordinate with a fourth, each response, however, contributing in its own fashion toward the common goal.

The overt activities, for example, involved in the pursuit of a long-standing drive, such as a young man's ambition and preparation for a medical career, are practically unlimited in number and variety. Many are quite indirect and incidental, such as deciding where to spend a week-end in view of the need of conserving his resources to pay his college bills; others, such as studying for a state examination, are direct. Yet, directly or indirectly, all such acts point to a common goal, which at the start is years distant. These innumerable acts

constitute a coherent and functionally unified organization of goal-directed behavior.

Another illustration of the integrated character of directional behavior is an individual's racial prejudice, which expresses itself in words, facial expressions, visceral responses, or overt acts, and may lead to the formation of elaborate rationalizations. It appears in the peculiar manner in which he perceives and thinks of members of the despised race; he has, we say, a 'stereotyped' concept of that race; and that stereotype in turn sets for him the meaning of everything he reads or hears about anyone of that race, as well as determining his words, feelings, and conduct on the subject. All these diverse responses thus have a common functional nature. Many different terms are employed to call attention to this disposition; besides 'stereotype' and 'prejudice,' we may call it a 'complex' (if unadmitted), or an 'aversion,' or a 'drive,' or a 'sentiment,' or an 'opinion.' It is all these.

D. Directional Dispositions Named by Their Goals

The characteristics of directional dispositions we have mentioned bring up the question of how they are to be designated, in thought or conversation. We cannot define a given attitude by specifying its neural mechanisms, for these mechanisms can neither be observed nor identified. We cannot designate a desire or an aversion as we do reflexes, by reference to the characteristic movement or motor structure involved, for their expressions are too numerous and varied. To refer to any disposition by formal S–R symbols is, of course, meaningless until its peripheral components have been identified in some other manner. Nor can we designate directional dispositions as we do traits or abilities by simple abstract terms.

We are thrown back, therefore, on the necessity of indicating any particular directional disposition in terms of the goal, object, or class of objects to which it points or refers, supplemented by some indication of the functional and dynamic trend of the responses. Its verbal designation is therefore more difficult and cumbersome than that of an ability or trait; we must employ descriptive phrases or even sentences,

rather than single words. In practice we often abbreviate the description to goal-reference, omitting qualifications. Thus, we speak generically of 'attitudes toward nationalism,' 'beliefs about telepathy,' 'stereotyped ideas of Communists,' 'desire for food,' 'sentiment about one's family,' 'interest in science,' 'taste in art,' 'the drive for personal power,' and the like, with little or no specification of their positive or negative or other qualitative characteristic. Sometimes the context serves to indicate the goal or object of reference without explicit mention of it. But, explicitly or implicitly, it is always the goal or object which gives name to the disposition. To say of a person, "He exhibits an attitude," or "He has an interest," is nonsense unless the context makes clear to what object it refers. Hence the significance of the word 'directional.' 'Direction' of a disposition, it is hardly necessary to add, does not imply spatial direction; although at times, as in measurement and correlation, it is convenient to represent directional variations by spatial symbolism.

When we characterize an individual by reference to his attitudes or other dispositions, we supplement the goal-reference by some indication of its nature. Thus, we describe a person in terms of 'his attitude of aversion toward nationalism,' 'his uncritical beliefs about telepathy,' 'his stereotype of Communists as riotous fanatics,' 'his epicurean taste in foods,' 'his lukewarm sentiments toward his family,' 'his technological interest in science,' 'his liking for string quartet music,' 'his vehement pacifism.' The supplementary qualifications usually specified are: 1. The general functional quality of the disposition; 2. Its adient or abient trend; and 3. Its dynamic or passive character. It is true, we do at times refer to 'Mrs. A's attitude toward religion,' 'Mr. B's interest in science,' or 'Miss C's taste in art,' without further qualification; but oftener than not the context implies or suggests it. Definite individualization of any disposition, however, calls in addition for as exact a quantitative statement as possible of its degree, intensity, frequency, favorableness, and perhaps other attributes. Of these variables, more presently.

E. Descriptive and Predictive Meanings of Directional Dispositions

Directional dispositions of every class are commonly used to characterize individuals, not only in popular speech, but in differential, applied, and social psychology, sociology, psychiatry, in fact in all phases of psychological and social sciences which employ concepts referring to the individual. Thus, we describe a person by saying: "he is interested in chemistry"; "he holds to the Catholic faith"; "he is opposed to fascism"; "he likes Brahms' music"; "he is prejudiced against Japanese"; "he thinks of golf as an old man's game"; "he wishes to win recognition as a writer"; "he always takes a second cup of coffee"; "he has a proclivity for chess"; "his aim is to get a Ph.D. degree."

In these and coordinate expressions, the implication is that this individual deviates from others in the respect named, and that this deviation not only distinguishes him, but is in some way significant enough to be employed as a mode of characterization. How much he deviates, and from what norm, is rarely specified, and is usually uncertain or unknown. Were these characteristics universal, they would be taken for granted, or at most mentioned casually as human characteristics, but not used to distinguish an individual. In such expressions there is also clear implication that not just one isolated act is intended, but a system of functionally related acts, to some extent recurrent, representative, and predictive.

Directional dispositions are not only as widely used as abilities and traits for describing individuals, but are for most purposes even more illuminating and useful, since they refer to the concrete content of activity. Anyone who has tried to interpret descriptions of persons or of personnel requirements as found in recommendations or occupational specifications knows how ambiguous, valueless, perhaps even misleading, such quality-descriptions can be which are couched in terms of abstract personality traits. What we call an individual's 'personality' or 'character' can never be adequately described without reference to *what* he does, thinks,

and feels, as well as to the manner and efficiency of his performance. Indeed, his various motives, interests, sentiments, attitudes, beliefs, etc. are not so much expressions of an underlying character as they are the very body and substance of that character.

Directional dispositions are also in increasing use as explanatory concepts, in accounting for many aspects of individual and collective behavior. Social psychologists and psychologically-minded sociologists employ 'social attitudes' and various special types of attitude in explaining such phenomena as variations in sentiment, practice, and belief from one cultural group or class to another, racial and religious prejudices, stereotyped social and personal concepts, in-group and out-group sentiments, public opinion, propaganda, etc., and give much attention to the study of the conditions under which such attitudes arise, change, and disappear. Industrial psychologists investigate and rate workers' attitudes, and use their findings in explaining vocational performance. Psychiatrists and abnormal and clinical psychologists employ elaborate techniques for revealing repressed sentiments and ideas, and thus discovering and altering patients' motives, as well as for explaining their peculiarities. Vocational psychologists test occupational and other interests as an important part of their analysis of vocational fitness.

Directional dispositions are used not only in description and explanation, but as bases for forecasting probable human behavior with respect to the goals and objects specified, both in the individual and the general population. Thereby they make possible, through control of determining conditions, some degree of control of behavior. Underlying this practice, of course, is the assumption of their relative permanence. Particular dispositions, like abilities and traits, vary widely in their frequency in the population. Some are universal, either because hereditary or (more likely) because formed under substantially universal environmental conditions. In either case, if we can discover the conditions of their occurrence, we can predict within limits the behavior of all members of the race or species. Other acquired dispositions, while not

universal, characterize the culture of large fractions of the population, such as a language or nationality group. Smaller culture groups of many kinds, such as occupational groups, sects, etc. provide further common bases of prediction with which most of us acquire some familiarity. Thus, intelligent people can predict fairly accurately the general type of answer a lawyer or a physician will give in reply to a question about law or health. We do not usually realize, until we pause to reflect, how large a part of our daily routine depends upon the knowledge of what other people are likely to do under these common conditions.

With dispositions which vary individually, the task becomes more complicated. We generalize, "men want to have money"; but some men want money intensely, while others are relatively indifferent to it; some want large amounts, while others are satisfied with just enough to get along. Prediction and control, particularly in the clinical and applied fields, thus necessitates familiarity both with individual peculiarities through the employment of standardized diagnostic techniques and with common human nature as well.

A common source of error in prediction arises from the practice of designating by the same name and assuming the identity of directional dispositions which really are not one but two or more distinct mechanisms. M likes mathematical generalization, but dislikes accurate computation; so that whether M says, "I like mathematics" or "I dislike mathematics," someone is sure to misunderstand him. Errors in judging an individual may result either from our failure to recognize that he differentiates two aspects of a situation or object which to us seems unitary, or the reverse. Or it may result from our failure to recognize that two modes of response may, through some genetic peculiarity, really be quite unrelated and inconsistent. A person's verbal statements, for example, may misrepresent his habitual conduct and his conduct belie his verbal opinions, if it happens that one was formed under one set of circumstances and the other under a radically different set. Or, sentiments acquired in childhood may lead one to feel and perhaps act in ways which mature

belief and assertion deny. Of course, such instances may be criticized as being 'unintegrated' and 'immature'; but much human behavior is just that, and must be so treated. It seems likely that controversies over the terms 'attitude' and 'opinion' may be partly due to failure to allow for this duality and disparity of origin and expression.

II

Having discussed our introductory propositions concerning the nature and psychological significance of directional dispositions, let us now consider them briefly with respect to certain additional features which characterize them, and particularly with respect to their relation to abilities and traits, the question of what is meant by 'general' attitudes, habits (etc.), the distinguishing features of the many classes of disposition, and the dimensions of variation which they exhibit and in terms of which they are measured.

Directional Dispositions, Abilities, Traits

Directional dispositions may be distinguished roughly from the other modes of characterization, abilities and traits, by saying that whereas an ability refers to whether and *how well* an individual does and can perform a given act, and a trait describes his constitutional *manner* of performing it, a directional disposition specifies *what kind of acts* he performs. The distinction lies not in the behavior, nor in the reactive mechanisms, for one and the same bit of behavior may be regarded either as the expression of an ability, a trait,[4] or a directional disposition of some type, depending on whether we regard it from the standpoint of its efficiency, its manner of performance, or its object of reference. The differences between the three are purely logical or conceptual.

In contrast to *abilities*, directional dispositions ignore the criterion of efficiency. We speak of a person's strong interest in art, or his liking for art, with no implication as to his ability either to execute or judge art objects. Of course, a positive correlation or causal connection may exist between an interest

and an ability, but this is a question of fact to be answered experimentally; logically, the two categories are distinct.

In contrast to *traits*, directional dispositions are limited by their objects, both with respect to the experiences which produce them, and their modes of expression. A behavior tendency which characterizes a person's reactions to so wide a range of things that it must be regarded as wholly a function of his constitution and not of the nature of the object, is properly classed as a general trait and not a directional disposition. Thus, the person with a strong interest in art may express that interest in behavior, which is either voluble or silent, laudatory or fault-finding, emotional or matter-of-fact, articulate or inarticulate, sociable or unsociable. Insofar as these characteristics, volubility, sociability, emotionality, etc. are exhibited in like degree in most of his other activities (and are not evaluated as abilities), they must be regarded as general traits and not as a part or function of his art interest.

Thus, practically any act can be regarded as the expression either of an ability, a trait, or a directional disposition, depending on the aspect emphasized. "He reads mystery stories" expresses a directional disposition; "He reads them avidly," a trait; "He reads them with shrewd penetration," an ability. Again: "He plays golf; he plays around 82; he plays with concentration," express the three aspects, as do also "His ambition is for personal power; he is the most powerful political figure in his state; he exercises power ruthlessly."

In some extreme cases it is not easy to decide whether the object of a disposition is sufficiently distinct and definite to justify its being called directional, or whether it should be regarded as a general trait. For example, masculinity or femininity of interest, altruism, sociability, egocentrism, æstheticism, or such vague characterizations as attitude toward the opposite sex, attitude toward life, or attitude toward reality, do possess a nominal definiteness of object which may perhaps entitle them to be called 'attitudes' and 'interests'; but it can well be argued that a constitutional disposition which points in almost all directions at once can hardly be called 'directional,' and that these should therefore

be regarded as traits, descriptive of a general manner of acting towards objects, as, indeed, they frequently are regarded. Other border-line examples were cited in the earlier article on trait-characterizations,[3] based on such deviate forms of behavior as that of liars, thieves, bluffers, philanthropists, heroes, and others. Such names as these have a double implication. They imply goals or objects of action, and they also imply manner of acting, so that it is a matter of the aspect emphasized whether we shall classify them as traits or as attitudes. Barring such extreme cases, the distinction between the two types of characterization is usually clear.

It is important to observe that each of the several classes of directional disposition can, like abilities and traits, be regarded from either of two points of view: 1. With emphasis on *differences;* 2. With emphasis on *similarities.* The first point of view is that of the differential, applied, and clinical psychologists. Sometimes applied psychologists distinguish and contrast the variations of a given act and their determining conditions, but oftener they are concerned with differences between one person and another. Such differences may be metric or they may not. The expression, 'individual differences,' has so generally been associated with psychometrics, that the importance of non-measured individual differences in personality patterns has often been disregarded, if not actually denied. Exceptions, of course, are psychiatrists, sociologists, and some social psychologists, who make use of non-metric case histories, characterizations, and type-classifications.

The second point of view (similarities), sometimes called the nomothetic point of view, is that of general and experimental psychology, which not only groups acts into classes and classes into categories on the basis of some similarity, but also undertakes to ascertain and measure the determining conditions of classes of acts and to describe human nature generically, in terms of common characteristics and common forms of response.

Both points of view, of course, are equally valid, necessary, and complementary to one another. The several approaches

[3] Carr & Kingsbury, *Op. cit.*

to the study of organized personality have often emphasized the differential aspects to the neglect of the nomothetic, stressing the fact of human variance, and not that of human likenesses. Some writers, in fact, give the impression of having forgotten that personality includes uniformities as well as individuality.

Generality of Directional Dispositions

One of the important distinctions among directional dispositions is in respect to generality. Some attitudes, opinions, sentiments, likes-dislikes, interests, etc. are said to be more general than others, perhaps because stronger, while others are characterized as specific, restricted, particular, individual, idiosyncratic or infrequent. At the same time, attitudes and habits are sometimes contrasted with traits by saying that the latter are general, the former are particular. Partly because of this confusion of practice, partly because of other inherent ambiguities, some comment on the use of the word 'general' in this connection is in place.

As was previously pointed out with respect to abilities and traits,[4] to characterize any one attribute, such as a belief, sentiment, prejudice, interest, liking, aversion, or motive as more or less general than another without specifying which of several meanings of generality is intended, is ambiguous and perhaps even misleading for descriptive purposes. Without undertaking to recommend a revised system of usage, however much it may be needed, we will merely point out several different meanings of generality.

Let us first mention that the word 'general' is very commonly used, whether intentionally or not, as synonymous with 'vague,' 'indefinite,' 'unanalyzed.' Withholding further comment on this practice, we can note at least four distinct legitimate meanings of the word which need to be clearly distinguished.

1. The first meaning is 'general in respect to the population.' As pointed out in the earlier discussions mentioned, the term 'universal' is here preferable. The fact of beliefs,

[4] Carr & Kingsbury, *Op. cit.*

sentiments, and motives common to practically all members of the race has at times been taken as evidence of their hereditary nature. More probably it is evidence of conditions in the social and physical environment which are substantially uniform for all individuals. This class of dispositions stands in sharpest contrast to those which are restricted to a single individual.

2. Generality of interests, sentiments, or prejudices may refer to the scope of some one class of objects. Since sports is inclusive of tennis, 'interest in sports' has greater generality than 'interest in tennis.' 'Liking for books' is more general than 'liking for fiction'; and 'antagonism toward war' than 'antagonism toward the Ethiopian War.' Sometimes, as previously pointed out, too great generality of this type leads to uncertainty as to whether a disposition is to be classed as directional or as a general trait. In contrast to this sense of generality are those attitudes, sentiments, etc. which refer to single, definite objects, persons, places, or activities.

3. A third meaning is generality in respect to the variety of modes of expression, as contrasted with restrictedness to a very few types of response. A general disposition, in this sense, is one whose goal is so inclusive or so remote in time that the acts which serve as means to its realization are very numerous, and perhaps related in a complex hierarchy, as in the case of what are called 'drives.' An interest in aviation, for example, which manifests itself in habitual reading about airplanes, examining airplanes, designing airplanes, riding in airplanes, flying airplanes, constructing airplanes, watching airplanes, and talking about airplanes, is more general in this sense than one which finds expression merely in watching airplanes. Again, the drive that ties the medical student down to a rigorous and prescribed program through years of academic and professional training is much more complex and general, in respect to the kinds of act it dictates, than is an urge which is simply and quickly satisfied.

4. A fourth sense of 'general' is in respect to frequency of occurrence. A disposition which is given expression at every opportunity may be characterized as general, although the

more common usage is likely to be, "He generally exhibits such-and-such an attitude," or "He is generally interested in that-sort-of-thing," or "He has the habit of generally doing so-and-so." This meaning of 'general' of course, referring to variations in time, place, and circumstance, sometimes coincides with the third and perhaps the second of the meanings mentioned, but it may be independent of either.

Varieties of Directional Disposition

Thus far we have considered only the features common to all named classes of directional disposition. We should like to know what distinguishes attitudes, opinions, sentiments, purposes, and the dozens of other types of disposition from one another.

We must bear in mind that these terms are of popular, not scientific origin; that they have sprung up wholly unsystematically in response to social recognitions of behavior distinctions which needed to be thought and talked about, and that the consequence is a nomenclature which is disorderly, even chaotic, full of synonyms and near-synonyms, of gaps and deficiencies. We need not expect, therefore, to do more than to suggest the more important types and dimensions of variation they express.

As previously mentioned, directional dispositions are inferred from observed behavior of various kinds and degrees of adaptive completeness. Psychologists have long found it convenient to classify mental acts into: 1. Those which terminate in some kind of perceptual or ideational apprehension of their object or goal; 2. Those which involve affective experience and evaluation; and 3. Those which lead to (or aim at) some change in the object or in the relations of the organism to the object, *i.e.*, cognitive, affective, and conative activities. It is true that these are almost invariably designations of abstract aspects of complex acts, so an act of any one of these kinds means an act in which that aspect is merely the most conspicuous. These designations are equally applicable to directional dispositions, and offer perhaps the most obvious way of classifying them.

Dispositions which are predominantly *cognitive* include opinions, beliefs (including dogmas, superstitions, and other special types), ideals, stereotypes, and what, for want of better names, we may call individual perceptual-sets and conceptual-sets. Some of these dispositions refer to peculiar habits of perceiving a given object; others, to ways of imagining and conceiving it; others, to habits of judgment and thought about it. Some convey special shades of meaning; thus, a 'stereotype' is not only a fixed way of picturing and conceiving an object, but one which is prematurely closed and hence oversimplified, inadequate, and fallacious, and which implies deprecation. An 'ideal' has goal-reference and an adient emotional tone, and suggests also some doubt of its easy attainability.

Three characteristics of cognitive dispositions are important. 1. Cognitive processes are not reproductions of objects "as they are," but are as subject to individual variability as all other mental processes; 2. Even though cognitive, they are far oftener irrational than rational; 3. While any particular object-disposition (*e.g.*, stereotype of communism, opinion about the church) can be represented as one or more linear variables, as it is, for example, in measurement of opinion, the number of dimensions of variation in the opinions, beliefs, or stereotypes about any object, and the abstract and limited character of such dimensions makes it simpler for most non-metric purposes to describe them in qualitative rather than quantitative terms.

Affective dispositions include sentiments, complexes (psychoanalytic), prepossessions and prejudices, likings and dislikes, loves and hatreds, desires and aversions, interests, predilections, preferences, and other equivalents and near-equivalents. All are characterized by feeling-tone, pleasant or unpleasant, which is in some manner referred to the object or goal. They differ from simple affects in having object-reference, and from both feelings and emotions in that they are not reactions but organic predispositions to reaction. The p–u dimensionality is explicit in most of these paired terms, and implicit in others, although emotional dispositions,

like cognitive dispositions, may need also to be described qualitatively. Some, such as desires, aversions, interests and the like, could equally correctly be classified as conative, while complexes and perhaps interest have cognitive aspects. 'Sentiment' is perhaps the most typical affective disposition; most of the others can be characterized as some form of sentiment. As with cognitive dispositions, special shades of meaning attach to some terms. 'Complexes' differ from other sentiments and affectively-toned ideas in being partly or wholly repressed. 'Prejudices' (and prepossessions) connote a bias, anti or pro, which, like stereotypes (with which they are substantially identical, save for emphasis) are indifferent or hostile to change, and hence imply a prematurely closed mind. 'Interest' usually refers to a rather broad class of objects or acts.

Terms designating *conative* dispositions are numerous and poorly differentiated. Some emphasize the goal or end toward which action is aimed; purposes, aims, goal-sets, ambitions, expectations, hopes, aspirations, anticipations, and innumerable other terms, reflect the influence which man's hopes have on his vocabulary. Most of these have also a distinct cognitive (imagination) aspect. Other terms stress action, either adient or abient, frequently with an affective tone such as colors most striving; among them are desires, wants, wishes, urges, will, cravings, lusts, appetites, hungers, needs, and so on. Still other terms suggest both goal and action, such as conations, motives, drives.

As with cognitive and affective dispositions, many conative terms convey some specialized or border-line meaning. 'Ambition,' for example, suggests a wished-for personal status; 'appetite,' 'lust,' and 'craving' suggest some strong organic disturbance, need, or other stimulus. A 'wish' suggests a fainter belief in the likelihood of attainment than does a hope, anticipation, or expectation. 'Drive,' as previously suggested, usually applies to long-time dispositions affecting large and inclusive areas of personal behavior, thus finding expression in complex hierarchies of acts. 'Motive' is perhaps as

indefinite (and hence useful) a term as any; in practice it means almost any kind of organic determinant of goals.

Besides the three classes mentioned, we should add a fourth and inclusive class, comparable to 'directional dispositions.' Several terms imply a directional readiness for response, but in a manner unspecified. 'Attitude' is at present most popular, especially with the social scientists; it has the merit of not being predominantly cognitive, affective, or conative, but all three; it suggests their non-rational character and social origin, but its connotation is more static and postural than dynamic; hence, our preference is for 'directional disposition.' 'Set,' 'goal-set,' 'end-set,' 'directional-set' and other variants have merits, but historically 'set' has usually meant a temporary *Aufgabe* or *Einstellung;* moreover, 'set' suggests something fixed and static. 'Habit,' as used by Dewey in his 'Human Nature and Conduct,' is substantially equivalent to 'attitude' or 'predisposition,' but the ambiguity of usage of the word is against it. Still other terms may be mentioned as used in this sense, among them, propensity, predisposition, bent, proneness, proclivity, and perhaps others, each with its overtone of meaning.

Dimensions of Variation in Directional Dispositions

Any directional disposition of whatever class may vary in one or more dimensions, from individual to individual, or in one individual from time to time. Theoretically, any respect in which one reaction can be rated as 'more' or 'less' than another, may serve as a dimension of variation. For example, opinions about the origin and nature of the church can be represented along a line ranging from the extreme opinion that it is a perfect, divinely instituted and guided, spiritual entity, to the opposite view that it is a purely arbitrary designation of some social group, between which extremes would be arrayed intermediate opinions, attributing to the church varying degrees of spiritual quality. Then, if numerical values are assigned to these opinions in some reliable manner, we have a scale for measuring any opinion on the subject. Whether or not it is in place to employ this highly abstract manner of

representing variations depends entirely on its value for the user's purposes. Whenever measurement is necessary, some dimension of variation must be isolated and graduated. For non-metric purposes, qualitative description may be more useful.

The dimensions in which directional dispositions vary are as numerous as one may choose to make them, but three general types of variability which have already been mentioned are of sufficient importance to warrant further discussion. The first may be termed the adient-abient dimension. Perhaps the most significant difference between directional acts and their underlying dispositions is that some are positive, approaching, pleasurable, adient, while others are negative, withdrawing, unpleasant, abient. The functional significance of this antithesis is obvious. Most acts, of course, range somewhere between the positive-negative extremes, but most fall far enough away from the neutral point to be characterized as more-or-less adient, more-or-less abient. Language provides many modes of expressing this dimension. We say, with respect to a person's directional disposition regarding object 'O,' that he has a drive toward O or a drive away from or in opposition to it; a desire for it or an aversion for it; that he finds it pleasant or unpleasant; that he likes it or dislikes it; is favorably disposed or antagonistic toward it; that it has a positive or negative value for him; that he seeks its continuation or its cessation; that he accepts or rejects it, believes it or disbelieves it, is interested in or bored by it; that he is pro or anti toward O; that his behavior is adient or abient with respect to O. This dimension more directly characterizes conative and affective dispositions than cognitive. Nevertheless, any opinion, belief, conceptual-set, value-judgment or other cognitive attitude can be phrased in such form as to call for assent or rejection, agreement or disagreement, and thus be measured on a scale of positive-negative valuations.

A second type of variability includes all the dimensions in which qualitative differences in cognitive or emotional dispositions can be expressed, in the manner just described. The

number of dimensions which may thus be selected for quantification is limited only by one's interest and patience in identifying them and giving them linear expression. Thus, one could construct many other scales of opinion about the church besides the one suggested; for example, opinions about its social value, its probable future, the changes it most needs, its importance in comparison with other institutions, its æsthetic and ethical values, one's own obligations toward it, the merits of the various sects, the significance and worth of the several sacraments, worship-features, or forms of organization and control, and so on indefinitely. Emotional attitudes toward the church likewise reveal several possible dimensions of variation—reverence, contempt, trust-fear, love-hatred, faith-suspicion, exaltation-depression, and so on. Certainly, there is no one exclusively correct way of measuring opinions or emotional attitudes with respect to the church.

A third dimension in which directional dispositions vary is in respect to their dynamic or static character. At one extreme are those which have no more effect than to contribute a slight directional bent to activities already going on; at the other, highly energized dispositions, typified by drives, craving, urges, loves, and hatreds, etc. This might be called the dimension of strength or intensity of dispositions. Since mental dispositions are conceptual things, there is little point to the controversy over whether an attitude is a source of energy or merely a directing tendency; it depends on one's concept of 'disposition.' It seems likely to prove less confusing if we conceive a disposition as a structural organization, distinct from but activated by the energy whose source is neuro-muscular metabolism. In this sense, the degree of dynamism is merely the degree in which the neural pattern is organized so as to facilitate motor discharge.

These three types of variation influence, to some extent, the term we select to designate a particular disposition. Wishes, for example, are less dynamic than cravings, interests than drives; an aversion for an object is abient, while a desire for it is adient.

Besides these, other less important dimensions can be

pointed out. The four meanings of 'generality' previously cited—universality vs. individuality, inclusive vs. specific objects, varied vs. uniform expressions, frequent vs. rare occurrence—can serve as bases for measurement if desired; so also could such attributes as relative permanence, ethical value, genetic maturity, conformity to standard type, and others.

Measurement of any directional disposition, as has been said, implies the abstraction of some single dimension of variation, its expression in graduated form, and the orderly assignment of scale-values to the several grades of performance. Although it is commonly assumed that what is measured is the amount or strength of the mental disposition, actually, as in all psychophysical measurement, units are physical, never psychical. Thus, an attitude scale does not measure directly the degree or amount of favorableness in one's mental set toward the object; the scale-values are assigned to a graded series of statements (or other objects) to each of which the subject is instructed to give or withhold his assent; and it is the average scale-value of the accepted items which is taken as the measure of his attitude. The same general principle holds true of all other disposition measurements. Animal motives are measured in terms of the relative amounts of punishment they will endure or obstacles they will overcome to attain the object; industrial motivation, by the amount of output under varying objective conditions—hours of work, methods of pay, type of supervision, working conditions, etc.—assumed to have varying incentive value; school motivation, by the amount of correct performance under varied conditions of reward, praise, reproof, penalties, awareness of success of one's efforts, social stimulation, etc. Occupational interests are measured by Strong in terms of the weighted frequency of 'like,' 'dislike' 'indifferent' responses to items in his schedule, as compared with the performance of standard occupational groups. Values are measured by Allport and Vernon and others in terms of the frequency of choice of alternative statements. Conversion of these physical quantities into units of adience-abience, intensity, conformity, or other psychical attribute,

is usually in terms of assumptions with respect to the frequency of the several degrees of such attributes in a normal population.

Summary

To summarize briefly our discussion: The psychological fact of selectivity in respect to objects and goals is one that requires to be explained systematically. Such explanation is primarily in terms of constitutional conditions, inferred from characteristic forms of behavior—conditions which we denote as dispositions, and which we differentiate and name in terms of the objects and goals with reference to which they are directed. Directional dispositions are thus explanatory concepts, and are in general use for describing, explaining, and predicting the reactive nature of individuals. These uses account for their importance in applied psychology; but because they also explain an important aspect of psychological activity they are no less important for systematic psychology. The same thing is true of abilities and traits. Since, however, the nomenclature both of particular dispositions and of various types of disposition is of popular origin, and hence full of ambiguities and contradictions, systematic psychology should undertake to develop a more appropriate terminology for dealing with them.

[MS. received October 24, 1938]

POSTURAL TENSIONS AND THE CONFLICT SITUATION [1]

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I

The experimental psychologist ventures with caution into provinces of psychiatry and abnormal behavior. Hence, a useful treatment of our topic first involves a definition of terms. By postural tension is meant the general bodily background (neuro-glandular-muscular) operating to sustain and energize the overt response pattern at any given moment.[2] The phrase 'conflict situation' is used to describe a field-structure exhibiting a strong tendency toward directed action and an inhibiting condition or frustration to the realization of the goal.[3] With these qualifications in mind, we shall discuss the proposition that many of the personality maladjustments found in situations of conflict may be studied experimentally as a relationship between the tonic or postural segment of response and its phasic or kinetic aspect. Accepting the principle that thwarting an activity-in-progress develops functional residues which influence subsequent phasic response, it will be argued that such residues are either derived from or expressed by postural tensions.

Practically all accounts of the dynamics of frustration behavior emphasize the functional significance of motor processes, but give little attention to their actual character. (32, 37, 38). Instead of a *complete* description of the en-

[1] Studies in Muscular Tension, series 2, no. 12. Read before the Society for Personality Study, Chicago, November, 1937.

[2] For further descriptions see other papers in this series, especially (16, 27 and 28).

[3] Many accounts of the dynamics of frustration behavior may be subsumed under this general description, including the 'psychic tensions' of psychoanalysis (32) and topological psychology (37), and the 'conditioning' (31) explanations of related experiments. We shall not be concerned here with contrasting our own psycho-physiological interpretations with views of a more strictly psychological nature.

tire behavior flux occasioned in the conflict situation, only the prominent phasic reactions are noted; the supporting processes of postural tension tend to be systematically ignored, partly because they are revealed by use of supplementary instruments and partly because their relation to the overt adjustment is not immediately obvious.

II

There is now good reason to believe that the character of overt reaction is largely determined by the background of excitation or pattern of postural tension met by the incoming stimulus (**5**, **16**). Such tensions comprise a type of effector activity which is distinguished from the more obvious phasic segment of response by its long latency and duration, its wide 'spread,' and its interoceptive (rather than exteroceptive) regulation. These properties, originally referred to the 'tonus' of skeletal muscle, have gradually been extended to other effectors, including smooth muscles and glands (**41**, **42**). Different control centers have been established for the tonic as opposed to the phasic type of response (**49**) and one authority would extend the differentiation through to the effector organ (**33**).

That the tonic action of muscle, gland, and nerve possesses dynamic functions in behavior has long been suspected and is now an established fact. Beginning with the older work on dynamogenesis (**14**, **15**) and continuing down to the definitive researches of Jacobson (**35**), Bills (**1**) and others (**17**), it has been shown that the experimental alteration of postural tension affects the quality and quantity of performance. Attention has also been given to the postural accompaniments of various higher nervous activities (**2**, **18**, **30**, **44**), including compensatory reactions due to distraction (**10**, **39**), loss of sleep (**19**), and other disruptive influences (**4**, **26**, **40**). Work has been done on the mutual interaction of the tonic and phasic segments of reaction (**18**) and preliminary excursions have indicated a wide range of individual differences in the postural aspects of complex behavior (**13**, **20**, **47**).

Such dynamic properties as are indicated above suggest

the importance of postural tension in many types of behavior exhibited in reaction to the conflict situation. This view was given elaborate formulation over a decade ago by the psychiatrist Kempf (36). The essentials of his theory are covered in two statements; (*a*) when the 'affective craving' of some part of the autonomic apparatus is thwarted—that is to say, not immediately supplied by 'projicient' or phasic reactions—"the postural tensions of that part is increased relative to other parts . . . and the flow of stimulation from it dominates the afferent flux from the remainder of the system, thereby dominating behavior"; (*b*) when such tensions remain unresolved for some time and "the individual gets set on a postural course like the exalted, the persecuted, the paranoic, and the melancholic," it is extremely difficult to break through the stereotyped pattern of autonomic excitation with exteroceptive stimulation.

These hypotheses have received a certain confirmation in experimental researches. Heightened muscular tensions found to accompany interruption of mental work are reduced by task resumption (22). There is a general rise in postural activity during inhibition of micturition (23) and this apparently influences acts not directly related to the retention of urine (25). Points of indecision in answering an attitude test show heightened bodily activity (3), and anxiety connected with school examinations produce postural changes in the digestive organs (45). Questions touching upon conflict of psychotic patients are accompanied by changes in blood pressure (9) and other postural adjustments (7, 35, 38). On the other hand, 'successful' solutions of arithmetic problems by normal subjects have been accompanied by greater (4) or the same (11) amount of muscular activity as have reported 'failure' reactions, where one might have expected greater tensions due to frustration. It appears from these conflicting results that before precise statements are possible, we shall have to know more about the limits of the influence which an existing tension level has upon reaction to new stimulation.

III

In spite of the general pertinence of the above approaches, it seems that the role of postural tension in frustration behavior is more adequately studied through use of the experimentally induced conflict situation. Here some basic tissue condition is prevented from attaining its usual and immediate channel of expression by the presence of other stimulation, and bodily changes are observed before, during, and after removal of the thwarting condition. Disregarding the means by which this general technique has been applied to lower animals, we shall deal exclusively with problems involved in the experimental 'loading' of human subjects. These problems include [1] the need for quantifying the inhibiting stimuli and the organic disposition which is being blocked, [2] the determination of suitable peripheral indices of tension levels and patterns, [3] the establishment of basal tension levels upon which background the ambivalent stimulus patterns are introduced and [4] the relation of postural changes under conflicting stimulation to the character of accompanying phasic reaction and to pertinent antecedents of individual history. An examination of these problems, together with certain preliminary results which are developing from their attack, may serve to indicate the general fruitfulness of the psycho-physiological approach to the conflict situation.

[1]. The experimentally induced conflict situation presents more difficult problems of control in men than in animals. For while a basic tissue condition such as hunger contraction or bladder distention may be prevented from attaining immediate equilibration by an externally administered punishment, in man at least we have also to contend with the operation of individual 'standards of achievement,' 'set' and 'levels of aspiration.' These subjective factors more or less over-rule the effects of the objectivily defined ambivalent stimulation; that is to say, the frustration involved in the inhibition or 'thwarting' of a basic organic disposition is largely a matter of how the subject relates his present performance to past behavior—his own and that of others around him.

In our early work with human subjects, we thought to induce conflict in a manner analogous to the development of experimental neuroses in animals. We controlled the basic tissue ('drive') condition of bladder distension by having the subject drink a standard amount of water and we introduced as an ambivalent 'thwart' stimulus the shock which would be received whenever urination was attempted.[4] We argued that when the micturitional urge became so strong as to overcome the threat of shock punishment, higher center control would be automatically released and urine discharged; micturition time would thus express the inherent ability of the individual to withstand conflicting patterns of stimulation without loss of integrative control.

We soon found that these assumptions were much too simple. For one thing, we discovered subjects reacting in terms of what they thought they 'ought to do'; we found many competing with one another and with their own records of previous trials, and what was especially significant, apparently deriving postural reinforcement for these higher-order motivational excitants. Moreover there was indication that the degree of subjectively estimated failure or success in earlier periods of stress carried over to influence the test performance. These facts brought about a revision in the experimental methodology which meets the quantification of 'thwart' and 'drive' stimulation at a more complex level and which is useful for frustration studies of animals as well as men.

The procedure makes a frontal attack on competitive tendencies by telling the subject in the first trial that we have a reliable test of his ability to maintain directional control over behavior while in a disequilibrating organic condition.

[4] The parallelism between this procedure and frustration studies with animals should be noted. Whether the goal is urination, food or mate, the tissue 'drive' and its 'thwart' are under systematic control. The 'load' in all cases is related to the degree of ambivalence between two competing patterns of excitation and not to the intensity of either one of these stimuli alone. As a measure of the ability to maintain higher level behavioral control under load, micturition time might correspond to number of crossings made in the animal obstruction technique or to the fineness of discrimination achieved in positive-negative conditioning before breakdown.

The threat of shock upon release of urine is still utilized and scores on sensory discrimination and other higher-order activities are recorded during the period of inhibition. This permits the setting of an 'uninstructed' level of performance. In the second trial, the subject is told that his behavior of the previous day (micturition time and performance scores under load) was below the average obtained by men of approximately the same age and level of intellectual maturity; these raised 'standards' are kept before the subject during the test, but no direct suggestion is made that he attain them and no special penalties are exacted for failure. In the third trial, average 'standards' are announced which are a given number of points below the performance attained in the first trial. On the basis of these three trials we are then able to determine the degree to which stimulation in the form of performance standards supplements the threat of shock in thwarting micturition. We are finding considerable postural carry-over from one test to another, with performance levels shifting as the thwart conditions are decreased or increased. It would seem, therefore, that instead of functioning exactly to balance out the thwarting effect of the arbitrary shock intensity,[5] postural activity falls below or rises above these just efficient limits. The factors responsible for such over and under compensation probably give an essential key to an understanding of individual reactions both in experimentally induced conflict situations and in those of every day life.[6] In

[5] The point has been made elsewhere in related problems; Morgan (39) mentioned the importance of the subject's 'congenial pace' in determining reactions under distraction, and Poffenberger (40) has maintained that organismic 'complacency' is not necessarily an attempt to reestablish metabolic equilibrium under disturbing conditions, but to make the performance correspond in so far as possible with previously conditioned standards.

[6] Grave difficulties are occasioned in interpreting individual reactions to such 'standard' experimental loadings. Differential patterns of postural tension are probably built up very early, so that certain tissues come to have lower thresholds of arousal in one person than in another (*cf.* Holt, E. D., *Animal drive and the learning process*, New York: Holt, 1931). If a disruptive situation (such as inhibition of micturition) involves a tissue which is already contributing to a so-called 'unconditioned' tension pattern or basic 'instinctive drive' in one person, but not in another, its measurement may tell little about relative ability to stand 'load' in general. Before any

fact, it may even be suggested that the different reactions of lower animals to the same objective barrier or thwart stimulus is partially affected by failure or success in attaining goals under other circumstances. Thus the degree of organic disequilibration is not X (tissue need) minus Y (externally conditioned inhibition), but X plus added facilitative conditions minus Y plus added inhibitory conditions.

[2]. The choice of suitable peripheral indicators of postural tension ushers in another set of problems which must be solved before the reaction dynamics of the conflict situation may be fully understood. It is recognized that the numerous tissue activities forming the postural substrate are differentially engaged in various individuals, and that use of records taken from any one tissue as symptomatic of the general tension level can be defended only upon proof of that tissue's reflection of spread effects from other sources. Current researchers employ several concomitant indicators, often without knowing which is the most reliable or physiologically significant measure of general reactivity. There has been considerable work on such methodological problems, but in situations so divergent as to make interpretation difficult. That is to say, the reliability of certain physiological measures has been studied in the basal metabolic state while others have been correlated in connection with less controlled stimulus situations.[7] Age of subject, period of observation, and type of recording equipment used have varied widely. In the determination of physiological significance matters are

one measure can be used as symptomatic of general capacity, several tests of loading will have to be run upon the same individuals and the intercorrelations determined. This may bring out a general loading factor, somewhat independent of the specific type of loading imposed.

[7] The multiplicity of possible measures of postural reactivity (including metabolism, respiration, blood change, skin resistance, action potentials, and more indirect and crude indices of visceral and skeletal change) make a study of their intercorrelations under identical conditions very difficult. For certain preliminary attempts, see Darrow, C. W., Electrical and circulatory changes to sensory and ideational stimuli, *J. exper. Psychol.*, 1929, 12, 282; Freeman, G. L., and Darrow, C. W., Insensible perspiration and galvanic skin responses, *Amer. J. Physiol.*, 1935, 111, 55–63; Freeman, G. L., and Giffin, L. L., The measurement of general reactivity levels under basal conditions, *J. gen. Psychol.* (in press).

somewhat better. Because palmar skin resistance apparently summates parasympathetic and sympathetic effects (**6**) as well as expresses an overflow response to skeletal muscle action (**8, 21, 46, 48**), this measure now seems to provide a significant single index of the total postural tension or general reactivity level. On the other hand, the extreme sensitiveness of this indicator makes for difficult interpretation without refined controls. Later work may show some cruder method will be more useful in rough determinations and clinical practice.

[3]. In determining the bodily energies mobilized in reaction to a conflict situation it is first necessary to know the level of organic reactivity from which such departure occurred. Early workers were very careless in this regard and in consequence much disagreement was reported as to the effect of various stimulating conditions on such indicators of energy change as the pulse rate, galvanic skin response, and muscular action potentials. It was Dodge (**12**) who first pointed out the absurdity of assuming that new stimulation and activity adds to the total energy mobilization its own quantum without reference to its relation to previous demands. As an instance of the need for establishing a standard basal condition prior to beginning experimental stimulation, Dodge cited the following example:

> In measuring the relative metabolism of walking and running by the pulse rate method, if one were running and started to walk the pulse rate would fall whereas if one were standing quietly and started to walk the pulse rate would rise. There is contradiction here only on the false assumption that running continues in undiminished force after walking begins—that the walking is a process that can be superadded to the running without inhibiting it. Positive and negative are absurd terms to use in this connection. Whatever indicator one uses, the two values find adequate expression only in a continuous series in which *completest practical relaxation* is the basis of comparison (**12**, p. 17).

Dodge's argument may be transferred to the problem of measuring postural tensions in experimentally induced con-

flict situations, where some organic disposition is thwarted from attaining equilibration through appropriate phasic reaction. Here the loading might be accompanied by a fall rather than a rise in postural activity, provided reactions occurring in the pre-stimulus period made greater demands upon the organism than did the stimulus disturbance. To eliminate such a possibility and to secure a standardization of immediate antecedent conditions, three factors may be brought under control; these are the subject's previous work activities, his degree of habituation to the recording apparatus and his reaction to the personality of the experimenter.

Our experience in the control and check of these factors has been most encouraging. It now seems that comparable basal conditions are most easily secured by early morning observations taken before the subject's induction into his regular daily routine. Control of food and water intake (as in B.M.R. tests) is necessary only when such items are to be involved in the experimental stimulation (as in inhibition of micturition). Fifteen to thirty minutes rest on a cot is usually sufficient to secure adequate relaxation from previous physical exercise. (If the naive subject is kept too long in this state, he may either go to sleep or become hyper-tense and irritated with the delay.) Our best check of such 'adequate' relaxation is in terms of the reactivity record itself; that is to say, when some indicator of general postural tension (such as the palmar skin resistance) levels off and shows no marked decrease or increase in activity for a period of five minutes, we usually take this as the 'basal' criterion for beginning the experimental stimulation. Effects of habituation are controlled by keeping the major parts of the physiological equipment out of sight and by placing the necessary recording apparatus upon the subject before the beginning of his relaxation period; records obtained the first day are always discarded and if the subject exhibits further apprehension or difficulty in relaxing under these conditions more training is given. Finally, since the presence of a prestige personality frequently occasions increased alertness and postural tension we have found it desirable to leave the

subject alone in the experimental room and to give instructions through a loud-speaker.[8]

[4]. Since all manner of supporting and ambivalent organic dispositions find reflection in the postural tensions of the moment, those established before, during and after the experimental conflict situation must be analyzed in reference to the character of accompanying phasic reaction and to functional residues of previous performance. Here, again, we may receive guidance from certain preliminary work. It has been shown that the increased bodily activity occasioned by superposing stimulation upon the basal condition of rest is sometimes accompanied by improvement in performance and sometimes by a loss of precision (**17**). In studying individual cases we have found evidence that these facilitative and inhibitory effects are often a function of the tension level from which the departure occurred (**28, 10**). There is reason to believe that the highly intelligent discriminative reaction accompanies only a limited range of postural activity (**27, 29**), and if these limits are individually set, most satisfactory adjustment to an experimentally induced conflict situation would typically be found in persons whose basal tension level is neither excessively high nor excessively low. Even more significant for diagnostic purposes is the recovery quotient or rate of return to physiological equilibrium following removal of the experimental load (**24**).

Since these differences in reactivity are usually as consistent as they are marked, we have been led to seek for the relatively persistent internal conditions which make for a hypo- or hyper-excitable reaction background. Here we have met with great systematic difficulty; for while we have found that the basal reactivity level expresses for some individuals an organic carry-over from some previously initiated frustration, others of approximately the same reactivity level have shown little or no evidence of residual conflict; and although we have observed a few instances of reduction in

[8] It is recognized that these methods do not 'relax away' the more remotely antecedent conditions which influence pre-stimulus reactivity level. Perhaps the solution here would be the use of subjects trained in Jacobson's technique of *progressive relaxation* (**35**).

postural activity as the conflict was resolved (as under successful psychoanalysis), we have been more impressed by the fact that the unresolved conflicts of the individual's past history will not themselves account for the wide range in basal levels of postural tension. A high reactivity level *per se* does not tell whether the individual is in this state due to postural residua of previous thwart stimulation or whether some more inherent condition (such as endocrine balance) makes for hypo- or hyper-reaction. We are now using a series of questions to explore the relation of conflict residues to tension levels exhibited prior to the experimental situation. On the assumption that greater postural tensions will accompany questions touching upon significant rather than non-significant items (**34**), the mean increment to such questioning is used as a reference number in correcting the previously established basal reactivity level for postural carry-over from other conflict situations.

IV

While it is difficult to predict the general significance which the study of postural tensions will have for problems of mental disorder—including breakdown under load—progress in research may be furthered if there is laid out a theoretical analysis of how the mobilization of organic resources affects resolution of the conflict situation. As a summary and extension of much desultory writing upon this subject, we can state the following tentative hypotheses:

1. Regarded physiologically, the organism is a dual mechanism, with the tonic reaction system supplying the energetics of activity and the superposed phasic reaction system supplying its direction and control. The tonic and phasic segments of response are mutually dependent, so that changes in the level of postural tension influence the character of overt adjustments which are sustained and *vice versa*.

2. The level of postural tension characteristic for a given individual in the 'basal' condition of rest is commensurate with the character of the most persisting internal conditions of stimulation. When forced beyond this level by the neces-

sities of growth, metabolism, or added stimulation, such increments in tonic action dynamize the superposed phasic reaction system which then tends to secure for the organism goals suitable to its needs. If frustration is encountered in this search, the heightened postural activity persists as an excitation pattern and exerts a constant pressure to be relieved by appropriate phasic response. In this manner, the basic reactivity level may be permanently or temporarily raised and its influence carried to a new situation.

3. Effective phasic adjustment is sustained by a limited range of postural activity. If tension levels exceed these limits, breakdown and disorganization of the higher order responses occurs. Here the balance of factors may favor 'withdrawal' from new and potential conflict situations and regression to behavior of less than appropriate energy requirement or some degree of 'overcompensation' and 'progression' to behavior characteristics of more extravagant than normal tension levels.

Though it would be unwarranted to suggest that postural tensions will of themselves provide a complete description of reactions to the conflict situation, the writer is encouraged to believe that we shall return from their study better equipped to handle the baffling problems of the psychoses. For while recognizing with Freud, Lewin and other students of strictly mental energetics that an aroused organic disposition affects behavior when prevented from attaining adequate equilibration, the fact that such dispositions alter postural activity and are in turn influenced by the level of activity which is met suggests new and relatively untried therapeutic measures.[9]

[9] No one yet knows the full importance of relaxation in reducing mental stress, and the alteration of the tension level by various mechanical and chemical means is relatively untried. Psychiatrists other than Kempf have seen this possibility. Thus, Janet (*The major symptoms of hysteria*, New York: Macmillan, 1929) recognized that the starting point for most neuroses "is an exhaustion of the higher functions of the encephalon . . . our nervous strength presents oscillations . . . and the lowering of nervous tension (as by great expenditure of nervous strength in emotion) brings about a general lowering of the highest functions." Franz (*Nervous and mental reeducation*, New York: Macmillan, 1924) remarks on the importance of 'kinesotherapy' in the reduction of mental conflicts, and Jacobson (35) has shown that training in general and local relaxation causes marked improvement in neuræsthenia and other disturbed forms of behavior.

It is possible that with adequate attention to detail, not only may we discover the special patterns of postural fixation which are early conditioned to unsatisfied needs and bring about their reduction by reconditioning, but also we may direct the general condition of hypo- or hyper-activity to levels most conducive to psychologically adequate behavior. Even more importantly, such analysis would open the way for the establishment of a psychiatric 'plimsoll mark'—a line such as is placed upon a boat to indicate how heavily it may be loaded and yet be buoyant enough to meet the added strain of storms. The psychiatrist of today is like a captain called upon the bridge after the ship has started to sink. Crucial stress has already weakened the patient and carried his postural adjustments near to or beyond the optimal point for highest-order response. If we had some objective criteria of the limits of effective performance set by persistent bodily conditions and could predict the quanta of energy which constituted the safe loading point of the individual's higher nervous centers, we might steer the hypo- or hyper-reactive into more appropriate situations before rather than after breakdown had occurred. Finally, instead of having to interpret a conflict situation in terms of mental dynamisms we might operate directly upon its postural residues.

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[MS. received November 9, 1938]

THE SPECIFIC PROCESSES CONSTITUTING THE LEARNING FUNCTION[1]

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The term learning, as psychologists use it, seems to cover any form of adaptation to a situation which an organism acquires. Despite the fact that the learning rates in different situations often show no correlation, psychologists continue to use the term learning for each situation and treat learning as a single function. Maier (**15, 17, 18, 19, 20**) has attempted to separate reasoning from learning and thus decrease the broadness of the term. Maier and Schneirla (**24**) have attempted to separate the formation of associations and the selection process, and treat them as independent mechanisms within a complex which constitutes the learning process. Krechevsky's work on hypotheses (**4, 5, 6**) has given rather clear justification for this division, and it is necessary to rely greatly upon his work in the analysis of the selection process.

Further analysis suggests that there are other processes which make up the learning function. In this paper I wish to designate specific mechanisms which can be separately measured, but which when taken together form a complex which constitutes the function of learning.

(1) *Variability.*—There seems to be a *need* for variability which like other *needs* determines an animal's preferences. Variability may aid in the mastery of a task because it causes the animal to display different behavior patterns. This need is very important in the selection process since behavior patterns must function before they can become associated with other events. In that it greatly determines the nature

[1] Communication from the animal laboratory supported by a grant from the Horace H. Rackham School of Graduate Studies of the University of Michigan.

of the animal's experiences it will determine the associative bonds which may be formed.

Krechevsky (7, 8, 9) has recently published three studies on variability in behavior. I wish to discuss these experiments in detail because of the implication they have for the theory of learning and because I wish to relate these findings with other data.

Variability or changes in behavior occur in problem situations. This variability is essential to the solving of perhaps any learning problem, but variability alone is not learning. As a task becomes mastered the variability decreases and certain behavior patterns become fixated. Krechevsky was interested in measuring this variability and did not wish to include changes resulting from fixation. He therefore used a rather free situation (Dashiell's checker board maze) in which there were 20 routes to food, all equally desirable in that all were equally rewarded and none was punished by excess distance.

Under these conditions normal rats varied their routes considerably whereas rats with cortical injuries limited their routes to a much greater extent. Thus normal rats on the average took 5.46 different routes in 15 runs, and operated rats on the average took only 3.00 different routes. Krechevsky believes that the tendency to take different routes depends upon the hypothesis of continuing in the forward direction, thus merely avoiding obstacles in the path. The obstacles are so placed that such a forward direction tendency makes several alternatives possible. I do not see the need for postulating a generalized response. The small area which the animals were required to cross could be learned as a whole, and "the animal might orient itself in this familiar area" without having formed a generalization. The ability of rats to reorganize spatial relations has been demonstrated when forward going tendencies were out of the question (15).

I would rather substitute for a generalized response the concept that in such a situation the rat has several particular routes available. Having learned various parts of the situation and having the ability to integrate experiences (15,

21), a number of particular routes to food can be constructed from the given elements.

Krechevsky, however, concluded that operated rats are less able to form the necessary generalization, and as a consequence their behavior is much more restricted. They are thus less capable of variability within the framework of their generalization.

But Krechevsky did not demonstrate a lack of ability in operated rats. He demonstrated that they choose fewer paths. Perhaps operated rats can but do not vary so much. If this is the case then the lack of variability in operated rats is due to a different kind of motivation. Previous writers have demonstrated variability in rats which occurs apparently because there is a need for change (15, 16, 14, 25). Perhaps operated rats do not need variety as much as do normal animals.

A loss in ability to vary behavior and a tendency not to seek variety in behavior may at first sight seem to be the same thing. I believe, however, that these two phrases imply real differences in basic processes. The following experimental situations illustrate how these alternative interpretations can be distinguished from each other.

1. If ability to vary is lost, then it follows that operated rats will be handicapped on my 3 table reasoning test since repeating an old response is always incorrect in the crucial tests. Maier (18) found operated rats handicapped, but with lesions as small as Krechevsky found crucial in his variability test he found no effect. If loss in variability means reduced ability to vary, the rats with small lesions should be handicapped. But if loss in a tendency to vary for the sake of variation is lost, then the animal can still vary its behavior if something else calls out the varying behavior.

In a more recent study Maier (22) found that rats which solved a problem before operation solved similar problems after operation as effectively as did normal rats. Analysis showed them to be as capable of variability as normal rats. If variability is a lost ability, it should not be available to such rats.

2. Suppose a group of rats was run 100 trials over a long path to food and 10 trials over a short. If the last series of trials was made on the long path, rats with small lesions should continue to take the long path because of their loss in ability to vary behavior. I believe tests would show a ready ability to shift on the part of rats with small lesions. Krechevsky's own findings which we shall discuss later support this belief.

3. The problem boxes on the whole make a greater demand on variable behavior than do mazes since the animal must go through a greater variety of responses to solve the former than the latter. The loss in ability to vary behavior should, therefore, handicap operated rats in problem boxes more than on mazes. The reverse is the actual finding. If, however, we regard Krechevsky's finding as demonstrating a loss in the preference for variety, then variety in behavior can still be called out by the proper stimulation.

Thus operated rats do learn problems very effectively when variability in behavior is demanded. They do not, however, use an excess of variability merely for its own sake. This, it seems to me, indicates not a loss in a process but merely a reduced tendency to express a kind of behavior the mechanism for which the animal continues to retain. Variety in behavior thus ceases to be an end, as in the case of normal rats, but still exists as a means.

In the study on detour behavior Krechevsky (**10**) drew a conclusion which I should likewise question because of his failure to distinguish between a loss in ability and a change in preference. He found that operated rats tended to make a detour less often than normal rats and concluded that there was therefore a loss in ability. His test was in reality one of a difference in preference and Maier's recent experiments (**22**) show no loss in such an ability.

Preferences are readily changed by brain injury as was shown by Krechevsky's (**5**) study in which he found a shift in the hypothesis used before and after operation. In this experiment he distinguished between preference and ability.

From my analysis it follows that lack of variation in behavior may be due to two different processes: (1) stereo-

typy in which the animal becomes a slave to a given mode of behavior, and (2) a preference for one mode of behavior over another. In the first case changing the alternatives does not free the animal but in the second case the non-varying behavior can be changed by presenting new alternatives. To change stereotyped behavior the fixed mode of behavior must be destroyed. It is because I believe both types of conditions arise that I emphasize the necessity of such distinctions.

In a second study, Krechevsky taught animals two routes to food. One route was varied from time to time; the other was kept constant and was somewhat shorter. No errors could be made since the maze contained no blinds. The animals merely learned to make a choice between two alternative routes to food.

When free choices were given, normal rats took the variable alternative 57.1 per cent of the time whereas operated rats took it only 34.3 per cent of the time. That the factor of variability rather than length was responsible for this difference was shown by the fact that a control experiment having a difference in length of pathway only but no variability difference resulted in 47.04 per cent choices for the longer by normal rats and 41.27 per cent for this path by operated rats. The introduction of variability in the longer path thus increased the tendency of normal rats to choose it (from 47 per cent to 57 per cent) and decreased the tendency of operated rats to choose it (from 41 per cent to 34 percent).

These results seem to show clearly that operated rats are less variable not because of the fixation of a given response through loss of ability to vary but by preference. Normal rats then seem to be motivated by the need for variation, and, although this need may sometimes be an advantage and sometimes a handicap, it should not be confused with the *ability* to form hypotheses. The operated rats learned two routes to food, but when given a free choice they tended to prefer the non-variable one; the normal rats also learned two routes but they preferred the variable path and when the paths were alike in variety they preferred to mix their runs. When Krechevsky stated that operated rats had lost plas-

ticity, he went beyond his facts. Rather the evidence points to a change in motivation, and this may result in more constant behavior. Because plasticity and preference are basically different mechanisms, this distinction is of utmost importance. The role of variety in behavior in counteracting the good performance of normal rats was demonstrated by the writer (16) in his study of attention. It was found that an increase in learning was associated with an increase rather than a decrease in errors. The tendency to vary overcame the influence of recency and frequency, both of which should have caused a reduction in errors.

The findings of Krechevsky's second study thus fit our interpretation that variability in behavior is based upon a need. Because Krechevsky thought of variability as an ability he was forced to postulate a second kind of variability to account for the results of his second study which showed that operated rats could but did not vary their behavior as much as did normals.[2]

In the third study Krechevsky repeats the second experiment with the variable and the standard paths, but the difference in length of these alternatives is reversed. The standard path is now definitely the longer. As a consequence the normal rats choose the variable path 55 per cent of the time or approximately the same as when it was the longer of the two (57.1 per cent), but the operated rats choose it 51.3 per cent of the time instead of the previous 34.3 per cent. Krechevsky points out that the normal and operated rats in this last situation are alike in their choices, but what seems more important is that the change has not altered the preference for normal rats but it has for operated rats. Variability thus alone determined the preference for normal rats and length of path was not a sufficiently determining motive to alter this preference. But operated rats, who ordinarily avoid variety, relatively speaking, responded to length of path, and so the

[2] Through private communications with Dr. Krechevsky we have reached substantial agreement on this point. Distinctions I have here emphasized were sometimes implied by Krechevsky but they were not elaborated sufficiently to make his meaning clear. What I have called a preference for variability he refers to as spontaneous variability in these communications.

tendency not to vary was neutralized by the tendency to take the shorter route which resulted in an equal number of runs on each path. The operated rats in this study, however, distributed their choices between the two alternative routes in a manner different from the normal. They showed more of a tendency to bunch their runs on one route, whereas the normals showed a greater tendency to mix their runs.

Our analysis has shown the important part which variability plays in learning. It can be measured separately and thus is not equivalent with learning; rather it must be regarded as part of the learning process. Since it seems to depend upon specific mechanisms and can be described by separate laws it is advisable to study it independently.

(2) *Plasticity.*—This term may be used to designate the animal's susceptibility to the formation of associations when other factors remain constant. The formation of a food association with a certain bit of behavior results in an acquired preference and this acquired preference then functions in opposition to the preference for variability. Definitions of learning usually describe the achievement of a new functional connection which results in new modes of behavior (*e.g.*, Cason, **2**). Different rates of achievement are regarded as measures of plasticity so that plasticity and learning are closely related in current thinking. Since the achievement of a new connection seems to depend on a variety of mechanisms we must question this close relationship unless we wish to exclude 'trial-and-error' behavior from the process of learning.

Plasticity can best be measured as a separate process by using situations which do not involve selection (trial-and-error) to any marked extent. The conditioned response technique is perhaps the best illustration. If the animal is alert and the stimuli are simple and intense so that perceptual and threshold differences are not determining factors, animals may be compared with respect to plasticity.

The unconditioned stimulus determines the kind of response which will be made, and as long as it is effective the response desired is at the experimenter's command. If a

neutral stimulus is presented against a uniform background (one which arouses no specific reactions) and if the animal is alert and in good health, the introduction of such a stimulus is likely to produce a dominant experience. The requirement is then largely one of associating the neutral stimulus with the unconditioned stimulus (or the reaction to it, whichever the case may be), and the readiness with which this is done becomes a measure of plasticity.

(3) *Perception.*—The organization of sensory data is also very important since it determines what stimuli will dominate and thus become associated. Animals with very similar sensory capacities may react to very different aspects of a situation (23).

Maier found in this study that if rats are presented with large and small black circles on gray backgrounds they are more likely to learn a response to a brightness difference if the small rather than the large circle is made positive and more likely to learn a response to a size difference if the large rather than the small circle is positive. In these cases the dominant aspect of the situation is determined by the positive stimulus. Individual differences are, however, very common and to explain these it must be supposed that the dominance is also a function of the individual. Kuo (12), in conditioning cats to fear rats, presented the rat and the box in conjunction with electric shock. It was found that for some cats it was the rat which became the conditioned stimulus, for others it was the box, and for still others it was the rat in the box. Which one became conditioned was determined by the cat rather than by the experimenter. Whenever complex stimuli are used the dominant sensory aspect is likely to be determined by the subject. Despite individual differences laws may be formulated describing the aspects of a stimulus situation which have the greatest likelihood of determining the animal's perceptual organization. Krechevsky (11) has recently demonstrated that the principle of proximity holds for rats as well as human beings. Lashley (13) has found that figure-ground relations function in rats in a manner similar to that found in man.

Thus all contiguous elements do not become associated, but the more dominant seem to be the most likely ones. Attention is the conscious side of this dominance. The law of effect describes the dominance producing aspect of a stimulus and for this reason becomes one of the factors in perception and in this manner influences the learning score of animals.

The method most frequently used in the study of perception is the method of equivalence developed by Klüver (**3**). Lashley (**13**), Krechevsky (**11**), and Maier (**23**) have utilized it in their studies of rat learning.

Often it is claimed that some animals fail to learn in a situation. Instead of failure to learn it is more likely that such animals learn something other than the experimenter intended. Such animals have been regarded as inferior learners. Before we can draw such a conclusion we must demonstrate that something else has not been learned in place of the habit tested by the criterion. Had Kuo concluded that all of his cats had not been conditioned because they did not all show fear reactions to the rat, he would have erroneously characterized the cats which feared the box.

(4) *Behavior repertoire.*—The number of reaction tendencies which an organism possesses should also be mentioned. It indicates the extent of an animal's repertoire of behavior (Krechevsky's hypotheses). The extent of a behavior repertoire is quite different from the frequency with which shifts in behavior are made. (The cat's reaction tendencies differ from a rat's in the way it reacts to a dangling string, and it therefore will master a string pulling problem more readily than a rat.) Stimulation, whether external (incentives) or internal (needs), determines which behavior patterns will be tapped (**26**, **27**), but the possibilities must be there before they can be called into play.

An animal's behavior repertoire can be studied by analyzing its reactions in a no-solution situation as has been demonstrated by Krechevsky's studies on hypothesis (**4**, **5**). He found that rats with cortical injuries displayed fewer kinds of behavior and that the modes of behavior displayed varied with the locus of the injury. Individuals also differed in the

variety of behavior patterns at their disposal. Although comparative studies have not been made, it is quite reasonable to suppose that marked and consistent species differences would be found. Kuo (12) has shown that structure plays an important role in making for species differences by showing how a cat's structure destines it to learn to kill rats. Cats, because of their structure, learn to kill rats, and rats learn to avoid cats. Because they learn different things when put together in the same situation does not demonstrate a different learning function. Likewise, the natively determined modes of behavior give different species a different background of behavior tendencies, and these greatly determine what an animal will achieve in learning situations.

(5) *Fixation.*—Like plasticity, fixation implies an acquired change in plastic tissue, but instead of indicating the readiness with which changes are made it signifies the continued function of a change already made. A fixed response thus gains preference through previous experience, and the acquired amount of preference may differ with the animal used. Fixation should not be confused with retention since a reduction in the latter implies a loss whereas a reduction in the former implies a shift in function.

Fixation may be studied separately by comparing different animals in their tendency to persist in an acquired mode of behavior after it ceases to be adaptive. Buytendijk (1) found that operated rats were less able to change a simple position habit than were normal rats. In the formation of position habits the groups showed very little difference, however. Thus the brain injury affected fixation to a greater degree than it affected plasticity. Maier (18) found that operated rats had a much greater tendency to repeat errors. In an unpublished experiment the same author found that even normal rats will develop persistent position habits in a no-solution situation and when the problem is then made soluble they will persist in the position habit for hundreds of trials and thus fail to solve a simple discrimination (unpublished data). An animal's learning score thus is influenced by fixations and even though it may show several modes of be-

havior it may make shifts less often. Fixations thus result in stereotyped behavior.

Perhaps reasoning should also be included in this list since in animals it is seldom differentiated from learning. Because I have already given my reasons for separating this process from learning (**17, 19, 20, 21, 22**) I shall not here enlarge upon this topic.

Conclusion

We have thus found that there is adequate experimental evidence for breaking up the learning function into a pattern of specific mechanisms. It seems that our studies of learning have reached a point where further analysis is sorely needed. Correlations between different learning problems give us nothing but correlations. Behind these correlations there are functioning mechanisms responsible for the magnitude of the correlation and it is these which are important to the science of psychology.

The fact that the correlations are low suggests that either all learning situations are not primarily tests of learning or that learning is the resultant of several separate processes. To say that we are dealing with different kinds of learning is to make the term so broad that it has little meaning unless it is accompanied by a descriptive adjective.

To continue to treat learning as if it were measured by almost any kind of situation in which the animal gradually did what the experimenter demanded of it has tradition to support it, but unless we settle down to the measurement of single variables we can progress but little farther.

In my selection of terms I have tried to be suggestive and do not wish to be regarded as dogmatic. Substitute terms may be used, but when used they should tend toward restriction in meaning rather than expansion. The trend in terminology should be away from connotative and toward denotative words if scientific communication is to be achieved.

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[MS. received November 30, 1938]

REMINISCENCE, SUBSTANCE LEARNING, AND INITIAL DIFFICULTY—A METHODOLOGICAL STUDY

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The traditional view of memory and learning holds that compound ideas are held together by bonds; in forgetting, these bonds decay or are loosened, weakened, or destroyed. The famous experiments of Ebbinghaus and of many other investigators have seemed to confirm this view. It has even been judged possible to graph the typical progress of forgetting, which is rapid at first and then slower and slower with the passage of time. Later experiments have shown that this forgetting curve is by no means universal or even typical. The experiments of Wolf (10), Gibson (9), Bartlett (2), and others have brought out changes in memory which cannot be explained as mere decay or simple loss; forgetting seems to be an active process or, at least, to be accompanied by one. Furthermore Ballard (1) among others has called attention to the phenomena of reminiscence in which there appears a gain in retention rather than the expected loss with the passage of time.

English, Welborn and Killian (7), approaching the problem of reminiscence and forgetting with new techniques of measurement, found that the Ebbinghaus-like curve appears to characterize ability to recognize what has been learned by rote (their so-called verbatim or V-scores) but not the 'sense' or substance of material which has been understandingly studied (their so-called summary or S-scores). These investigators found with college students that even several months after the learning had taken place, not less, but more, of the substance was recognizable.

This distinction between rote and sense learning was found

consistently despite changes in a considerable number of variables. Welborn (7) varied the spacing of readings, Killian (7), the number of readings and the degree of mastery, English and Edwards (4, 6), the maturity level of the subjects, the measuring instrument, and the rehearsal element afforded by interpolated tests. In each of these studies retention was measured after various intervals ranging from 10 minutes to 90 days. These series of experiments with substance learning have to date used over 3500 subjects, and in all cases there has been a persistent association between the particular measure of retention, either verbatim or summary,[1] and a typical trend, in the retention curve, whether forgetting or reminiscence (respectively).

It thus appeared that with the passage of time, two opposed tendencies affect retention: one destructive; the other, not merely conservative, but actually constructive. The first tendency would seem to be associated with the rote memory which for practical and technical reasons has been the stock-in-trade of memory experiment. The second seems associated with 'intelligent understanding' or 'meaningful learning.' The 'common' forgetting curve appears to be a blend of both tendencies with the 'destructive' in the ascendent.[2]

In all of these studies, however, comparisons were in terms of medians and means of groups. But reminiscence and forgetting are not group, but rather individual and personal processes. The use of averages, here as elsewhere,

[1] More fully described in earlier reports (4, 6, 7).

[2] Bartlett's experiments are frequently—and properly—cited as an example of the advantage which accrues when we cease an excessive fractionation of the phenomena. Save in the closely related *Aussage* experiments, such transformations of memory as are revealed in his work had been overlooked—mainly because memory was studied in terms of 'atoms.' Admitting the validity of the criticism, we may point out the values also of considerable fractionation. Thus, in the series of experiments of which this report is part, a narrative or exposition is broken down into sentences which are regarded as independent measures of retention. But the fractionation is not carried to the point where the overall view is lost. We hope in the near future to trace experimentally the relation between Bartlett's transformation and the kind of reminiscence disclosed in our results.

may conceal as much as they reveal.[3] In a check investigation, however, English (5) showed that more *individuals* manifest reminiscence with S-items than with V-items.

The analysis, nonetheless, must be pressed still further. Reminiscence has been defined by implication as a net gain in a composite measure at subsequent testings with reference to a first test or base. That is to say, we subtract the subsequent measure or score from the earlier. If the first score is larger, we say that the subject has forgotten; if the second is larger, we speak of reminiscence. But what is really 'retained' or not 'retained' by the individual is the particular operationally defined *response* required. Thus the use of a composite score for each individual, no less than of averages, obscures what is actually going on in terms of processes. If we are to discover the conditions for reminiscence and forgetting, we must consider the *particular responses* which manifest the phenomena and not some hodge-podge net resultant. Accordingly we undertook in this study the analysis of individual item responses on repeated testings.

Procedure

First let us set the problem. A group of 30 students read and heard read an unfamiliar passage. Immediately thereafter they were given a test to show retention of the material and were retested at 60 and again at 90 days.[4] The test (reliability coefficient .82 ± .02), which has been described in other studies (7), was scored for 20 V and 20 S items. Comparisons between the means by the usual critical ratio technique give us Table 1.

Briefly, here is a measure of retention made up of two sets of items, constructed to be different in kind (or to be on a continuum and sufficiently remote from each other to constitute distinct sets). Comparison of the means reveals that

[3] Indeed it is entirely possible that reminiscence remained undiscovered so long chiefly because the data of most learning experiments are reported in terms of averages, where the net loss is the resultant of much gain and more loss.

[4] The tests were administered with the kind cooperation of Dr. J. E. Janney of Western College.

TABLE 1[5]

A Comparison of the Means of V and S Items over 90 Days
(N = 30)

	Mean Scores			Critical Ratios	
	Immediate (0 Days)	60 Days	90 Days	For Losses	For Gains
V	15.5 ± .25	13.8 ± .32	13.2 ± .40	$V_0 - V_{60}$ 4.1	$S_0 - S_{60}$ 1.7
S	9.5 ± .33	10.3 ± .32	11.1 ± .32	$V_{60} - V_{90}$ 1.2	$S_{60} - S_{90}$ 1.8
				$V_0 - V_{90}$ 4.9	$S_0 - S_{90}$ 3.5

with passage of time, there is a differential effect upon the two kinds of items. There is a significant drop in the *average* number of V-items retained from the first to the second test, while there is no drop in the *average* number of S-items from the first to the second test, but instead, a slight gain. These tendencies are even more marked if we take the change from the first to the third test. Following the customary line of interpretation, these results seem to give strong and unambiguous indication that reminiscence is associated with the S-items and forgetting with the V-items. Let us see, however, what we find when we consider particular responses.

Analysis of Individual Responses

The identical 40-item test having been given three times: T_0 (immediate), T_{60} (60 day), T_{90} (90 day), and each response to an item being either right or wrong at any of the three testings, we may have, for any item, 8 permutations or patterns of responses: RRR, RRW, RWW, RWR, WWW, WWR, WRR, and WRW. Our 30 subjects give 1200 pattern responses distributed among the 8 distinct permutations, 600 for the S-items, and 600 for the V-items.[6] The frequencies for each pattern were tabulated and are presented in Table 2.

[5] The data in this table have been reported in an earlier study (4).

[6] The scoring and tabulating of the twelve hundred patterns—which is no small task—was carried out by W.P.A. workers under the supervision of Miss Juanita Lilliedale.

TABLE 2

FREQUENCY OF PATTERNS OF RESPONSE

Pattern	V	S	Difference in Favor of V
RRR	295	155	+ 140
WWW	60	148	− 88
RWR	47	53	− 6
WRW	23	39	− 16
WWR	17	46	− 29 Reminiscence
WRR	35	79	− 44 Reminiscence
RRW	62	32	+ 30 Forgetting
RWW	61	48	+ 13 Forgetting

If to reminisce is to respond successfully where one had previously failed (no known practice intervening), then patterns WRR and WWR are those which display this phenomenon. Similarly, patterns RWW and RRW show forgetting.

The patterns RRR and WWW are not relevant since neither shows either variation. The patterns RWR and WRW are more perplexing. They would seem to indicate that both reminiscence and forgetting are at work (which is reasonable in all cases) and give no evidence for a preponderant effect of either. They thus cannot be used to measure a tendency toward either reminscence or forgetting.[7] We cannot find rational distinction between WWR and WRR in respect to the amount of reminiscence, nor between RWW and RRW in respect to the amount of forgetting, despite the apparent difference in the symbols; hence we treat each of these pairs of patterns as one.

A glance at the critical patterns for reminiscence, WWR and WRR, shows clearly the superiority of the S-item response. And *per contra* the patterns which show forgetting, RRW and RWW, are preponderantly composed of V-item responses. *Even when we have regard to the particular response to a particular item, we still find an association or correlation between V-items and forgetting, and between S-items and remi-*

[7] If we were to use these patterns, we should have to put them together in a zero category midway between reminiscence and forgetting. Moreover, in view of the small number of individuals who manifest these patterns, it seems very likely that these categories represent mainly chance.

niscence. The respective differences are so large and the number of responses considered so ample, that a calculation for statistical significance would be mere pedantry.

The Factor of Initial Difficulty

By definition, however, reminiscence and forgetting are both relative to an earlier response. Now as Table 2 clearly shows, more V-items than S-items were marked right on the immediate test, and since there are more V-responses which are right, there is greater opportunity for going wrong thereafter—that is to say, for forgetting. Similarly since there are more S-responses which are wrong on the immediate test, there is greater opportunity for recovery thereafter—that is to say, for reminiscence. It thus seems entirely possible that the association which has been so laboriously demonstrated in so many experiments, between reminiscence and that factor which differentiates S from V, is merely due to the intrusion of the factor of differential initial difficulty.[8]

Recourse to percentages as in Table 3 was not, moreover, very reassuring. The percentage differences between V and S are small, and one of the two is in the 'wrong' direction!

TABLE 3

Percentage of Possible Change

Right on First Test	Per Cent Later Forgotten	Wrong on First Test	Per Cent Later Reminisced
V 465	26	135	39
S 288	28	312	41

Analysis of the Variance

The next step under such circumstances was to eliminate the effect of the intruded factor of initial difficulty. All our experience, however, had indicated that S-items were inherently more difficult than V-items. Experimental equation for difficulty therefore seemed ruled out. No obvious way of utilizing partial correlation coefficients presented itself, without making assumptions which were at least ques-

[8] Indeed, the whole phenomenon of reminiscence is called in question. Is it due largely to chance factors as Brown (3) had previously suggested?

tionable. Analysis of variance and use of chi-square, however, seemed hopeful.

We first determined the initial difficulty of the items, working on the usual assumption that an equal frequency of errors (on the first testing) is a valid index. In computing this index, S- and V-items are thrown together since the null hypothesis, which is to be tested, holds that they are the same except in difficulty. (Distributing according to difficulty without regard to S or V should exhaust the variance if the null hypothesis is valid.)

We thus obtain three categories or variables: (*a*) the patterns of response (RRW, WRR, etc.); (*b*) the V- or S-classification of items; and (*c*) the degree of difficulty of an item (divided into quartiles as D_1, D_2, D_3, D_4). The 1200 responses are distributed according to these three variables in Table 4.

TABLE 4

DISTRIBUTION BY PATTERN, DIFFICULTY AND TYPE OF ITEM

Pattern	S					V				
	Difficulty Quartiles					Difficulty Quartiles				
	D_1	D_2	D_3	D_4		D_1	D_2	D_3	D_4	
RRR—Total	27.00	71.00	34.00	23.00	155.00	9.00	16.00	96.00	174.00	295.00
Per Item	3.38	8.88	11.33	23.00	**46.59**	4.50	8.00	13.71	19.33	**45.54**
WWW—Total	98.00	45.00	5.00	0.00	148.00	23.00	11.00	21.00	5.00	60.00
Per Item	12.25	5.63	1.67	0.00	**19.55**	11.50	5.50	3.00	.56	**20.56**
RRW—Total	7.00	13.00	10.00	2.00	32.00	4.00	5.00	30.00	23.00	62.00
Per Item	.88	1.63	3.33	2.00	**7.84**	2.00	2.50	4.29	2.56	**11.35**
RWW—Total	18.00	20.00	10.00	0.00	48.00	4.00	10.00	16.00	31.00	61.00
Per Item	2.25	2.50	3.33	0.00	**8.08**	2.00	5.00	2.29	3.44	**12.73**
RWR—Total	9.00	27.00	14.00	3.00	53.00	1.00	5.00	19.00	22.00	47.00
Per Item	1.13	3.38	4.67	3.00	**12.18**	.50	2.50	2.71	2.44	**8.15**
WRW—Total	18.00	14.00	7.00	0.00	39.00	4.00	5.00	8.00	6.00	23.00
Per Item	2.25	1.75	2.33	0.00	**6.33**	2.00	2.50	1.14	.67	**6.31**
WWR—Total	19.00	21.00	5.00	1.00	46.00	6.00	3.00	5.00	3.00	17.00
Per Item	2.38	2.63	1.67	1.00	**7.68**	3.00	1.50	.71	.33	**5.54**
WRR—Total	44.00	29.00	5.00	1.00	79.00	9.00	5.00	15.00	6.00	35.00
Per Item	5.50	3.63	1.67	1.00	**11.80**	4.50	2.50	2.14	.67	**9.81**
Distribution of 40 Items	8	8	3	1		2	2	7	9	

The total frequency in each cell shows the total number of responses per pattern in each classification (S or V) at each level of difficulty for all 30 subjects. Thus in the first row, there are 27 times when an S-item of first quartile difficulty is correctly responded to at all three testings. As shown by the bottom row, there are 8 S-items in this quartile, so the average number of times a perfect score is got with such an item is 3.38. (This is essentially the number of persons who make a perfect score with such an item and is to be compared with 30, the total possible.) The one S-item of fourth quartile difficulty was so easy that twenty-three persons responded correctly each time and no one (see the WWW row) missed it entirely. Again in the WWW row, 23 responses are tabulated for the two most difficult (D_1) V-items. That is to say, an average of 11.50 persons (out of 30) flunked the item every time they tried it.

As may be seen from the last row in Table 4, 'Distribution of 40 Items,' the 20 items in each classification are most unequally divided according to difficulty. But when we sum the per item entries across the row in each classification, by ignoring the number of items in each cell, we give equal weight to the four quartiles and thus approximate the distribution according to pattern which would be obtained if the twenty in each classification were equally frequent at each difficulty level. The figures in bold face type in columns 6 and 11 of Table 4 thus represent the S- and V-scores for each pattern when difficulty is a constant.

We know, of course, that S provides more of the former pattern and less of the latter than V. However, if the equating of items for difficulty has been properly done as described, this difference between S and V should disappear. This is just what happens within the limits of experimental error (indeed with a slight over-correction due to dropping of fractions and similar errors; compare the equated average figures for S and V respectively in the RRR row of 45.59 and 45.54 and in the WWW row of 19.55 and 20.56).

We have pointed out above that the critical patterns for forgetting are RRW and RWW and for reminiscence are

WWR and WRR. It is now possible to compare the dichotomy of forgetting and reminiscence with the dichotomy of V and S, difficulty being held constant. This is done in Table 5 which is obtained by simple summation from Table 4.

From inspection it can be seen that there is a decided correlation between forgetting and V in contrast with the correlation between reminiscence and S. By means of the chi-square formula, we may determine whether this association is statistically significant or due to chance. With one degree of freedom (which is our case) a chi-square of 3.8 yields a probability of significant association of 95/100 which

TABLE 5

ASSOCIATION BETWEEN FORGETTING-VERBATIM AND REMINISCENCE-SUMMARY, DIFFICULTY CONSTANT

Frequency Per Item

	S	V
Forgetting = RRW + RWW	15.92	24.08
Reminiscence = WWR + WRR	19.48	15.35

Fisher (**8**) considers sufficient. The above association, however, yields a chi-square of 9.7 which is so far beyond the minimum requirement that it is not even given in Fisher's table.[9]

Of course the chi-square test proves nothing as to the cause of the association nor does the size of the index indicate the strength of association or the degree of correlation. It merely tests for chance. But our treatment, we believe, eliminates the variance due to differences in initial difficulty and leaves a correlation which can scarcely by any possibility be attributed to chance.

[9] In applying this formula, each of the entries of Table 5 must be multiplied by $\frac{20}{4}$ since what we have is the average *per item* response; what we need for chi-square is the *total* number of responses. Multiplying the *average* in each of the four columns of Table 4 by $\frac{20}{4}$ instead of by the separate frequencies for each row yields the same total number of responses but gives equal weight to each column (which represents a difficulty quartile).

Conclusion and Comment

We conclude, then, that a definite correlation does exist between forgetting and whatever is specifically and differentially measured by the V-items, and between reminiscence and that which is differentially measured by the S-items. This association does not seem attributable to chance or to differences in the initial difficulty of the two kinds of items. We are thus back to the conclusion reached in previous studies.

It may seem a waste of time to prove what looked obvious on its face. But that obviousness was deceptive. At every step of our analysis it was genuinely possible that the empirical data should tell a different story, and it was only when this analysis was completed that it was possible to be assured of our conclusion.

The interpretation of that conclusion and its bearing on learning theory have been briefly indicated at the beginning of this article and more fully in others of this series. Here let us be content with this statement: there is an influence (or influences) at work in our two different but similar-appearing measures of learning, which produce remarkably consistent differences in retention parameters under very diverse conditions. Such differences are a challenge to our experimental ingenuity and our theoretical subtlety.

The significance of this report, however, we take to be methodological. We first reported the kind of differences here considered at the 1933 meeting of the American Psychological Association and in print in 1934 (7). They have attracted a certain amount of interest. To our knowledge, however, no one has challenged their validity. No one called attention to the very real possibility that our conclusion was vitiated by the unanalyzed effect of initial difficulty. A conclusion based upon inadequate reasoning was allowed to stand without criticism. Fortunately, more adequate reasoning substantiates the original conclusion.

Our methodological conclusion may be stated thus: wherever a difference relative to initially differing bases is in

question, the cause of the relative difference may be wholly or in part due to whatever differentiates the bases rather than to the independent variable of the experimental set-up. The usual critical ratio criterion is of no value at this juncture; but the analysis of the variance as set forth in this article provides one way to get around the difficulty.

And now a final word. How many other research studies (not alone, moreover, of memory) are infected by an inadequacy of analysis of similar character? It would be an easy but invidious task to call the roll. Thankful, however, that we have been allowed to find the beam in our own eye, we prefer to leave it to others also to cast out their own motes—or beams.

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[MS. received November 8, 1938]

THE NATURE OF THE EXTINCTIVE PROCESS

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Extinction, or the decrement and final disappearance of a conditioned response upon repeated applications of the conditioned without the conditioning stimulus, is no doubt the most obviously unique characteristic of conditioning. As such, it seems to have affected different psychologists in totally different ways. To Thorndike (**110**, p. 110) it serves as evidence that the facts of conditioning do not 'belong' to the main body of learning, since "ordinary learned reactions do not act in this way . . . conditioning is not a 'prototype' of learning" but a 'special case.' To Tolman (**111**, p. 324–325) it means that conditioning is a sign-gestalt-expectation phenomenon that "goes off only when and insofar as S_2 has tended in the animal's recent past to be a sign for the coming of S_1." On the other hand, Cason (**15**) used this 'decrement by repetition' as a telling argument in his well-known criticism of the traditional 'law of use,' while Dunlap in the formulation of his (**25**, p. 360) *gamma-postulate*, that "response decreases the probability of the same stimulus-pattern producing the same response," might well have been influenced by Pavlov's findings.[1] Finally, to Hull and his students (**43–47**; **65**; **107–108**) extinction has been the main middle term of their theorizing and 'logical deductions.' As a negative phase of conditioning it has become in their hands a concept no less tractable than ambivalence in psychoanalysis, and would indeed have to be deduced if it did not already exist.

Parallel with this use of extinction in arraying conditioning with other forms of learning, there has been in the literature—

[1] Dunlap himself disclaims that his postulate was suggested by earlier formulations.

both Russian and American—considerable discussion of the nature of the extinctive process itself. It is with this discussion that the present article is primarily concerned. Specifically, it will (1) review and re-state existing theories of extinction, (2) marshal and re-align all the known behavioral facts of extinction, (3) analyze and evaluate the theories in the light of these facts, and (4) attempt to evolve from these facts and theories some synthetic working hypothesis. More generally, the article will also consider (5) the question of the failure of extinction to manifest itself in a number of typical conditioning experiments and (6) the problem of extinction and extinction-like phenomena in learning and behavior that are not usually subsumed under the heading of conditioning.

Theories of Extinction

The first and foremost theory of extinction is of course Pavlov's own 'internal inhibition,' the assumption that non-reinforcement (or excessive reinforcement) results in an inhibitory state which produces the extinction decrement. The use of inhibition in this sense was questioned by Beritoff (**8**), Howell (**41**), Lashley (**63**), and others on the grounds that this term should be reserved for decrements caused by stimuli other than those of the response decreased,[2] and again by Beritoff (**10**) in that its supposed latency and duration are so many times greater than those of inhibitions previously studied by neurophysiologists. Pavlov, however, was apparently much less concerned with tradition than with constructing a hypothesis for a large number of facts that he thought he could not otherwise explain. His inhibition is in general quite unorthodox, being conceived as exclusively cortical, cellular rather than interneuronic, and *inherent* in any form of conditioned excitation. "The inhibitory process

[2] Howell: "An inhibition like an excitation presupposes a special stimulus and one asks in this case what is the stimulus that arouses the process of inhibition . . unless one assumes that the animal's growing consciousness of the lack of the reality supplies the occasion for the inhibition" (p. 152).

Lashley: "The term inhibition is meaningless in this connection since we are not dealing with the suppression of one activity by another which is all that inhibition means today" (p. 552).

arises in the nerve cells themselves and not in the connecting path. . . . The cortical cells under the influence of the conditioned stimulus always tend to pass, though sometimes very slowly, into a state of inhibition" (**85**, p. 234 and 391; *cf.* Meltzer: "Each and every state of life . . . is a resultant of the combination of two antagonistic factors, excitation and inhibition"). Still, as a new hypothesis, applied to a new level of behavior and operating with new facts, Pavlov's view can hardly be attacked merely for its non-conformity with previous assumptions and findings or for a possible misuse of a term. Rather, the main problem is whether *all* the extinction phenomena discovered by Pavlov and repeatedly confirmed by other investigators could be made to fit other simpler concepts. This task, as will be seen later, is by no means an easy undertaking.

The second major theory of extinction is that of Beritoff (**9**, **10**). Beritoff recognizes two factors in extinction, a general and a specific. The general factor is a kind of response fatigue resulting from the repeated stimulations; it is the factor that lies at the core of secondary extinction, or the extinction of a CR to one stimulus upon its extinction to another. The specific factor is postulated as reverse conditioning. It is contended—and experimental evidence fully supports this contention (**10**, **11**, **82**)—that in any CR situation there are established not only direct connections from the brain center of the conditioned to that of the conditioning stimulus—bell→ shock—but also reverse connections of the centers of shock→ bell. Therefore, in extinction, as the shock center becomes weakened by response fatigue, the excitability of the auditory or the bell center becomes relatively heightened and the reverse connections gain the upper hand. Extinction, in other words, is the reversing or the undoing of the effect of conditioning consisting mainly in restoring the *original* response to the conditioned stimulus. Beritoff's theory has a number of salient points in its favor. Its chief apparent difficulty is the concept of response fatigue which is here in reality a misnomer for some form of adaptation, since the intervals in extinction experiments are as a rule much longer

than those which produce true fatigue and since extinction, unlike fatigue, is greater with weaker than with stronger stimuli. However, even if some adaptation concept is substituted for fatigue, the significant problem still remains as to the mechanism of this adaptation.

Another theory of extinction is the one suggested by Winsor (**119**) and by Guthrie (**32**) that the decrement is produced by a change in attitude or affectivity resulting from the withholding of the conditioning stimulus. Winsor combines his view of a change in affectivity with a postulated shifting between parasympathetic and sympathetic neural patterns, and Guthrie, like Beritoff, separates extinction from adaptation (negative adaptation). Unlike the latter, however, Guthrie not only considers extinction to be the less permanent and adaptation the more permanent factor of change, but also identifies adaptation with conditioned inhibition and inhibitory conditioning in general.[3] Guthrie's complete theory for a prolonged series of extinction would seem to mean that at the beginning the CR is decreased because of a changed attitude and that later this decrease is enhanced by the conversion of the conditioned stimulus into an 'inhibitor' or a 'stopper,' signifying 'what not to do' or 'what else to do.' An apparent shortcoming of this theory consists in making extinction, which is a rather universal and constant phenomenon in phylogeny and ontogeny, depend upon something so variable as a change in attitude or affectivity. Extinction curves of crustacea and frogs have been quite similar to those of dogs and young children, and curves obtained from withholding food have been practically identical with those derived from withholding an acid or a drug, and yet it is difficult to conceive of the attitudinal and affective changes being alike in all these cases. In the writer's opinion attitudes are valuable concepts in accounting for the heterodox rather than for the orthodox manifestations of conditioning,

[3] Guthrie's use of 'conditioned inhibition' differs from traditional Russian usage. To the Russians 'conditioned inhibition' covers only those cases in which a stimulus comes to hinder another conditioned stimulus from action, and there are always two stimuli in such a CR situation, while Guthrie employs the term for any instance in which a stimulus comes to signal the cessation of some one response.

or less exactly, in explaining 'what conditioning does not do' rather than 'what it actually does.'

Closely akin to the preceding theory is the one of 'substitute response and competing behavior systems.' It has been observed by Figurin and Denisova (**28**) and by Levikova and Nevymakova (**67**) that few-months-old infants do not by any means become 'inhibited' during extinction trials. On the contrary, they respond with all sorts of voci-motor avoiding or 'protesting' reactions and very often the avoiding reactions follow incipient approaching responses. Similar but less pronounced observations were noted by Aronovich and Khotin (**2**) in monkeys. The writer called attention to the significance of these experiments in casting doubt upon the doctrine of internal inhibition (**90**, 75–76). He furthermore incorporated the notion of competing behavior systems in his general theory of 'dominance,' although he does not hold such competition *alone* sufficient to account for *all* experimental extinction. More recently this view was taken up by Wendt (**112**) who had confirmed on a larger scale the Russian experiments and who proffers 'competition of behavior systems' as an all-explaining extension of reciprocal innervation at the spinal level. Extinction is "a *redirection* of activity into other response systems rather than a generalized *reduction* of activity . . . an activity is inhibited, when some other reaction system takes its place" (**112**, p. 269 and 280). As expounded by Wendt, this theory of extinction suffers from at least three weaknesses. (1) It does not account either for secondary extinction, or for the usual negative correlations between the rates of conditioning and extinction. (2) It has no uniform nor clear explanation for the *initial* decrement and shift of dominance in extinction. (3) It must make very arbitrary and contorted assumptions in the many cases of extinction in which accompanying increments in other reactions are not evident.

A fifth theory of extinction was developed by Culler and his associates (**12**, **28**, **19**). These investigators hold that in any CR situation the conditioning stimulus plays a dual role, one of determining the specific pattern of the response to be

conditioned and the other of providing a general incentive to actuate the conditioned response-pattern. In extinction the CR pattern itself is actually intact but is inactive for want of motivation. It lacks the incentive role of the conditioning stimulus and thus sustains a decrement. Without denying the importance of motivation, it is still hard to see how such a general theory can square with the specificity and regularity of extinction, its permanent loss and partial recovery, its spread and negative acceleration, and so forth. Culler himself has apparently realized some of its inadequacies. In his recent paper (**19**, p. 151) he cites the experiment of Brogden that shows that reconditioning is harder after prolonged extinction series and explains this result by 'inverse conditioning,' a concept similar to the 'inhibitory conditioning' of Guthrie. "The bell without shock becomes ever more a safety-signal (stay *on* the grid!), even as, when combined with shock, it becomes an ever stronger danger-signal (get *off* the grid!)".

Wenger (**115**) advances the idea that extinction may be due to progressive muscular relaxation. He admits the existence of a genuine kind of extinction which he ascribes to conditioning to a competing reaction, but argues that the Pavlovian variety with its extinctive irradiation, spontaneous recovery, and disinhibition is an experimental artifact rather than a true result of learning. Spontaneous recovery means 'returning to a normal level of tension,' disinhibition is the outcome of the 'rise in skeletal tension upon an application of an extra novel stimulus,' and extinctive irradiation, or secondary extinction, follows from the 'general response decrement induced by the relaxation.' It is difficult to umpire this Pavlov-Wenger controversy, inasmuch as Pavlov repeatedly describes muscular relaxation as an accompaniment of extinction, and the question is 'which is the effect' and 'which is the cause.' However, from the literature it rather appears that Wenger's proposition is quite far-fetched, even though there is no doubt that muscular relaxation and tension do produce a number of pseudo-effects in conditioning. Decrement by repetition, spontaneous recovery, and disinhibi-

tion (or incremental distraction) have been observed under so many and varying conditions and set-ups—spinal level, crustacea, human adults under fatigue, dyspnoea and apnoea, and hypnosis (**1**, **13**, **31**, **53**, **57**, **78**, **79**, **99**), to cite only a few instances—that they clearly seem to be manifestations of the conditioning itself rather than of any state of the organism.[4]

The writer (**90**) proposed an asynchronization theory of extinction and negative adaptation according to which repeated *isolated* activation of a response results in progressively wider asynchronicity of its nerve units and consequently in a continuous weakening and dropping out of its effector units. The theory was launched as a speculation in neural terms, a special deduction from a general theory of 'dominance,' but has recently gained solid experimental support from the studies of Prosser and Hunter (**89**) and Hunter (**52**) who found by means of an action current technique that extinction "involves the gradual dropping out of active units and diminution in the duration of their activity" (**89**, p. 617). However, the writer's neural concepts—which are, to be sure, something that can be finally settled only by experiments of the Hunter and Prosser type—were by no means intended to cover his behavioral views of extinction. These views will only be named here, since they will be discussed fully later in the article. They are: (1) 'decrement by repetition' is the most universal factor of extinction, a factor which extinction shares in some way with habituation and adaptation; (2) reverse conditioning—restoration of the original response to the conditioned stimulus—is the general contributing factor of extinction, which distinguishes extinction from habituation and from adaptation; (3) redirected conditioning—conditioning to some action arising in the course of extinction—is a contributing factor that often but not always modifies the extinctive process; (4) changes in affectivity, attitude, muscular tension, motivation, and the

[4] Ilina and Lykhina (**53**) found that accelerating or retarding one's breathing decreased simple CRs, but had no effect upon differential CRs. Abramovich and Pichugin (**1**) and Kantorovich (**57**) discovered that lifting weights, squeezing a dynamometer, or running 300–500 meters did not affect the conditioning of ten human subjects.

like are only modifying organismic states and are not constitutive of the extinctive process *per se*.

Behavioral Facts of Extinction

1. The main phenomena of extinction, spontaneous recovery, and disinhibition (incremental distraction) have been observed and studied in animals as low as crustacea.

To date Mikhailoff's (78–80) experiment with 23 crustaceans and cephalopods is the most extensive study in the number of extinction series tested. His main findings on recovery, permanent loss of extinction, lesser extinction with spaced trials, correlation between rate of extinction and rate of conditioning have been strikingly confirmed by recent American investigators (35, 40, 50, 81, 93, 104, 117, 122).

2. Extinction has been noted and studied in infants a few months old.

Kantrow's (58) investigation of 16 infants is particularly illuminating in showing the emergence of both reverse and redirected conditioning in the course of the extinction. As the extinction progressed and the conditioned sucks at the sound of the buzzer decreased, the crying activities during its activation gradually approached and finally exceeded those of the control periods.

3. Extinction has fully manifested itself in decorticated animals.

Girden, Mettler, Finch, and Culler (31) report ready extinction in a decorticated dog and similar results were obtained by Beritoff (9) in decorticated pigeons and by Leutsky (66) in decorticated frogs. Prosser and Hunter (89) found extinction-like decrements, recovery, and disinhibition in reflexes of spinal rats and cats. They state that they were unable to extinguish a knee-jerk in a spinal cat after 980 trials, but that in the same animals "a reflex in which internuncial neurones are involved was extinguished in 27 trials" and conclude that "extinction and disinhibition occur beyond the sensory neurone and ahead of the final motor neurone, presumably at some of the internuncial synapses" (p. 616). In this connection it might be mentioned that Dodge (24) did not succeed in habituating the knee-jerk but was able to reduce the wink reflex to one-third of its amplitude and that Dusser de Barenne and McCulloch (27, 28) observed an extinction-like phenomenon—decrements at stimulation intervals of 13 seconds that became greater with weaker stimuli —in the motor cortex of three monkeys.

4. The chief manifestations of extinction have been observed in voluntary or 'instructional' CRs.

The many findings from Ivanov-Smolensky's and Bekhterev's laboratories were recently corroborated by Rexroad (95, 96).[5]

5. Extinction and its characteristic attributes have been shown to operate in CRs with drugs as the conditioning stimuli.

Kleitman and Crisler (59) confirmed earlier Russian observations, and since then a variety of similar observations have been made.

6. The degree of extinction is inversely proportional to the age and strength of the CR extinguished, the older and stronger the CR the less the extinction.

Russian observations have been confirmed by Williams (117) and by Youtz (121) with a large number of rats and with the bar-depressing-and-food-getting technique. Williams' curves of extinction as functions of the number of reinforced trials are negatively accelerated and fit the equation of $E = 62 - 62 \times 10^{-.017R}$ where 62 is a constant, E the median number of responses in the extinction series and R the median number of reinforcements in the training series. The inverse relationship between extinction and number of reinforced trials is not materially affected by the CR decrements which often result from excessive reinforcement.

7. Delayed and trace (successive) CRs are more readily extinguished than simultaneous CRs.

This generalization of Zavadsky (124), Dobrovolsky (22), Belitz (7), Lavrova (64) and others is apparently more than a special case of 5, the general relation between strength of CR and extinction. It is, however, in need of more exact confirmation.

8. CRs of higher order are more easily extinguished than those of the first order.

Brogden and Culler (12) found that a CR of the first order required four times as many non-reinforced applications for extinction as a CR of the second, third, or fourth order. Their experiment is, however, somewhat complicated by their application of an extra stimulus (or incentive) together with the higher order conditioned stimuli. Presumably, it is the communality of the extra stimulus that produced the extinction equality of the second, third, and fourth order CRs. (Even though the extra stimulus was ineffective by itself to elicit a CR, it probably was not without conditioning

[5] In order not to encumber unduly the bibliography, Russian experiments will be cited individually only when they have not been confirmed by American investigators; otherwise, they will be referred as 'experiments from some specified laboratory' or merely as 'Russian experiments.'

attributes.) Some Russian investigators (83) noted that CRs of higher order may be of greater magnitude than CRs of the first order.

9. Transfer CRs (CRs acquired by virtue of the conditioning of some stimulus) show more extinction than original CRs.

Liddell (68) and Hovland (40) confirmed earlier Russian findings.

10. CRs to patterns of stimuli are considerably more resistant to extinction than CRs to single stimuli.

Razran (92) found this relationship while working with salivary CRs and with spatial and tonal patterns; some evidence was also obtained that, other things being equal, extinction is inversely proportional to stimulus complexity.

11. The nature of the response conditioned may itself be a strong determiner of the degree of extinction.

In animals salivary CRs are much more easily extinguished than withdrawing CRs from nocuous stimuli. In the wink response Hilgard and Campbell (36) noted that 10 extinguishing trials undid temporarily the work of 90 reinforcements, while in Youtz's study the ratio of extinction responses to reinforcement was 1.2–1.7. In general, however, it may be justifiable to state that, other things being equal, responses involving widespread bodily action are more resistant to extinction than responses concentrated in a few effectors, but even this statement has notable exceptions (4, 42, 91).

12. Correlations between rate of extinction and rate of conditioning have been mostly negative: rapid conditioners are slow extinguishers and slow conditioners rapid extinguishers.

Negative correlations have been obtained by Mikhailoff (78, 79) in crustacea, by Hunter and by Youtz (50, 121) in rats, by most Russian investigators with dogs, by Bernstein and by Miller and Cole (11, 80) in adult human subjects with the wink response, by Wickens and by some Russian experimenters with withdrawing from shock in adult human subjects (116). Positive correlations have been reported by some Russian experimenters with salivation in dogs, by Hilgard and Marquis (35) with the wink response of four dogs, by Youtz (122) in rats when the extinction trials were made 24 hours instead of immediately after the training, and by most Russian investigators who had used the voluntary or shock technique with adult human subjects. In the writer's opinion, the negative correlations express the *natural* CR relationship and the positive correlations are due to some extra disturbing influence.

The first disturbing influence is central or subjective control. In experiments with school children negative correlations have been more pronounced in younger than in older children, and in the writer's own investigations the control of subjective factors has transformed zero into negative correlations. A second disturbing factor is probably related to overlearning which may in some way account for the results of Hilgard and Marquis, of Youtz, and of some Russian animal studies. A third possibility of explaining the discrepancies in the correlations is the Russian typological interpretation in terms of inhibitable, excitable, labile, and inert types. Inhibitable and excitable subjects—slow conditioners and rapid extinguishers, and rapid conditioners and slow extinguishers—produce negative correlations, while labile and inert subjects—rapid conditioners and rapid extinguishers, and slow conditioners and slow extinguishers—manifest positive correlations. In this connection Hunter's statement may be quoted that "rats that condition quickly may have either quick or slow extinction but rats which condition slowly have their responses extinguished quickly" (50).

13. The main curve of extinction, the CR decrements in successive non-reinforced applications, has typically been negatively accelerated.

The notable exception here is the rise or increment in CRs in the first extinction trials which has been observed by many experimenters; Hovland (39) has, however, demonstrated that these increments result from excessive over-conditioning. Another deviation is the wave-like form of curves of extinction reported by a great many other investigators, but Skinner (101) has shown that these waves are the outcome of emotional disturbances and they smooth out in subsequent extinction series. Finally, the initial plateaus and linearities of some extinction curves may also well be the effects of some overmastery of the conditioning task.

14. Extinguished CRs recover after a lapse of time, but as a rule the recovery is only partial and appears to be negatively accelerated.

Full recoveries have been observed, but they seem to be exceptional, although perhaps genuine with certain combinations of time, intervals, degree of extinction, degree of conditioning, and kind of response. Negative acceleration of recovery in time means that *additional* recoveries become progressively smaller in equal units of time. Russian observations have been corroborated by Miller and Stevenson (82) whose extinction study may, however, be considered by some psychologists unorthodox inasmuch as it consisted of removing a reward in the running of a maze.

15. Extinction recovery is proportional to the degree of extinction.

Hovland (40) and Razran (94) confirmed Russian observations. Razran tested the salivary CRs of 12 subjects 48 hours after 6 different degrees of extinction and found the recovery to be about 50–70 per cent of the amount of the extinction. More exactly his equation was $R = E^{.84} + .85N$ where R = amount of recovery, E = amount of extinguished portion of the CR, and N = its non-extinguished portion. Hovland's results with a 24 hour interval and with the GSR indicate recovery to be 65 per cent of the degree of extinction.

16. The slope of the curve of recovery from the extinction of CRs is considerably steeper than the slope of the curve of forgetting of CRs, or, up to a certain time, the recovery of the extinguished portion of a CR is significantly greater than the rate of forgetting of its non-extinguished portion.

Razran's results show (94) that after 48 hours the recovery is from 3.6 to 4.0 times as large as the forgetting, while Hovland's data which fit an equation of 65 per cent recovery and 15 per cent forgetting show an even larger recovery forgetting ratio. Russian studies have pointed in the same direction. As expressed by Pavlov " . . . the process of internal inhibition is more labile (unstable) than that of stimulation" (86, p. 138).

17. Up to a certain limit, the rate of extinction is inversely proportional to the lengths of the intervals between the extinction trials.

This generalization follows from statement 14 on recovery; it has been empirically confirmed by Mikhailoff (78–80), Hilgard and Marquis (36) and others.

18. In successive extinction series the number of non-reinforced trials required to produce complete extinctions progressively decreases, the decrease being negatively accelerated.

This generalization follows theoretically from statements 14 and 15 on the partial recovery of extinction and the proportionality of recovery to amount of extinction. The general decrease in non-reinforced trials has been confirmed by many investigators, but the negative acceleration with equal time intervals between extinction series has not been very evident (78–79).

19. When a CR to one stimulus has become extinguished, CRs to other similar stimuli may thereby become spontaneously extinguished, the amount of this 'secondary extinction' being a function of the similarity between the extinguished and the non-extinguished CRs.

In this country Bass and Hull (6) and Hovland (40) have in the main confirmed this theoretically significant attribute of extinction. Numerous experiments of this kind have been performed in all Soviet CR laboratories. It is striking that a similar phenomenon has been noted by Peak (87) with verbal associations and by Coombs (16) with the non-conditioned adaptation to the GSR.

20. Secondary extinction recovers much more readily and much more fully than primary extinction.

This relation which was found by Horn (38) and Degtiareva (22) is not a special case of the relation of recovery to the degree of extinction (statement 15) but a qualitatively new property of secondary extinction.

21. Sedatives increase extinction and stimulants decrease it.

Russian studies have partly been corroborated by Switzer (109) and by Skinner and Heron (105) with caffein and benzedrine.

22. Extinguished CRs may be restored (disinhibited) upon the application of some extra stimulus.

Switzer (108), Hunter (50), Wenger (114), Hovland (40), and Razran (93) demonstrated this phenomenon in this country; Skinner (103), however, failed to obtain it.

23. The extinction-restoring (disinhibiting) power of an extra stimulus is a function of its intensity.

Russian laboratories have usually reported that neither very weak nor very strong extra stimuli produce the maximum of restoration. Wenger (114) found a strong stimulus to be more effective than a weak one.

24. The extinction-restoring power of an extra stimulus is directly proportional to the degree of extinction, and the magnitude of a CR resulting from the application of an extra stimulus together with the conditioned stimulus of a partially extinguished CR is the algebraic sum of (*a*) the magnitude of the non-extinguished portion of the CR, (*b*) the decrement produced by the extra stimulus upon this portion, and (*c*) the increment produced by the extra stimulus in restoring the extinguished portion of the CR. The increment rate (*c*) is greater than the decrement rate (*b*).

Razran (93) tested the effect of a buzzer upon a CR to a light on 24 subjects with 12 different degrees of extinction and found the

resulting CR to be equal in most cases to 60 per cent of the non-extinguished portion of the CR plus 90 per cent of its extinguished portion.

25. Extinguished CRs are greatly restored merely by a separate application of the conditioning stimulus (without the conditioned stimulus).

The Culler laboratory and Schlosberg (97, 98) have confirmed Russian findings.

26. Extinguished CRs are rapidly reconditioned when the stimuli, whose effect has been extinguished, are recombined with the conditioning stimuli; the rapidity of this reconditioning is, however, inversely proportional to the degree of the extinction preceding it.

Numerous Russian investigations have demonstrated this generalization; Skinner (101–104), Hilgard (35–38), Razran (92), and others have partly corroborated it.

27. The course of extinction may be accompanied by a considerable amount of agitated behavior of the organism undergoing the extinction; the agitated behavior does not, however, appear to correlate significantly with the extinction decrement.

Considerable agitated behavior during extinction has been observed in infants (Russian laboratories, Kantrow, 58); in monkeys (Aronovich and Khotin, 1; Wendt, 112); in rats (Miller and Stevenson, 82) and in dogs by some Russian experimenters. The statement that 'agitated behavior does not correlate significantly with extinction decrement' follows Miller and Stevenson (82) who found this correlation to be −0.10 in one group of rats and 0.37 in another group.

28. Extinction may be overlearned or proceed below zero; and its behavior does not then differ qualitatively from that of ordinary supra-zero extinction; it is only more potent and the degree of its greater potency is directly proportional to the degree of its sub-zero extinction.

This is a common finding in Russian laboratories (10, 54, 55, 85, 86).

29. A CR decrement resembling in all respects the decrement of extinction may be produced by excessive or by un-

usually massed reinforcements of the conditioned by the conditioning stimulus.

Schlosberg (97), Hovland (39) and others have confirmed this generalization, previously made by a large number of Russian investigators.

30. Complete failure of extinction has been noted in a number of cases.

The notable instances are: (*a*) Baker (4) conditioning the pupil to subliminal stimuli; (*b*) Hudgins (42) forming pupillary CRs to verbal stimuli; (*c*) Hilgard and Marquis (37) with wink responses in monkeys; (*d*) Harlow and Stagner (34) with skeletal CRs in animals under curare; (*e*) Baschmurin and Muehlberg (5) in decorticated animals (extinction not impossible but extremely difficult); (*f*) Menzies (75) with vasomotor to verbal, auditory, and visual stimuli; (*g*) Kriazhev (61) with animals in a social situation (two dogs experimented on simultaneously; extinction not impossible but difficult); (*h*) Osipova (84) and others with volunatry CRs in school children; and (*i*) Razran (91) with salivary CRs in adult human subjects. Of these, Razran was able to account for his results in terms of disturbing subjective attitudes, on the ground that when these attitudes were controlled extinction was fully manifest (92–94). Razran's explanation may perhaps also apply to the findings of Osipova and even to those of Menzies and Kriazhev. However, other failures to produce extinction remain to be explained.

31. Extinction-like phenomena—decrement by repetition, more decrement with weaker than with stronger stimuli, spread of decrement to similar stimuli, increment after a lapse of time and upon extra stimulation—have been noted in verbal associations, non-conditioned responses, and spinal reflexes.

The study of Peak and Deese (87) with verbal material and that of Prosser and Hunter (89) with spinal reflexes are to date the only experiments of their kind. The work of Coombs (16) with the GSR and with auditory stimuli, of Seward and Seward (100) with various reactions to electrical shock, and of Halstead (33) with post-rotational nystagmus may be taken as examples of extinction-like manifestations in non-conditioned responses.

Criticism of Theories of Extinction and Facts of Extinction

The thirty-one factual statements of extinction presented here cannot of course be considered final. There is, however, little doubt that they cannot be ignored. In fact they are at

present the raw material, the brick and the mortar out of which any theory of extinction must be fashioned. Past practice of setting up all-embracing CR theories on the basis of studying or experimenting with a single response in a single organism can hardly be commended, as it only leads—to say the least—to a confusion of terms and meanings. Some of the inadequacies of such parochial theorizing have already been indicated in the first section of this article when the several theories of extinction were first set forth. Other shortcomings must have become clear upon reading the facts of extinction. It is only necessary now to bring the theories and the facts more closely together and to evaluate the theories in terms of the facts in a more explicit and organized manner. Since it is better for the evaluation of the theories that they should be considered in a reverse order from that of their original presentation (with the exception of the writer's own theory) Wenger's theory will thus be taken up first.

The Wenger hypothesis that spontaneous recovery, secondary extinction, and disinhibition—and *Pavlovian* extinction in general—are results of changes in muscular tension and "experimental artifacts as far as the conditioning process itself is concerned," has in the light of the above facts lost practically all its tenability. First, it was shown that in at least two experiments (**1**, **57**) considerable muscular exertion—lifting weights, squeezing a dynamometer, and running—had little effect upon the CRs of ten human subjects and that in a third study (**53**) retarding or accelerating one's breathing was similarly ineffective. Second, it was clearly substantiated that spontaneous recovery is a definite function of the time elapsed between extinction and recovery tests, which is a result quite contrary to Wenger's statement that 'the length of the time interval becomes unimportant' (p. 309). Third, both spontaneous recovery and disinhibition were found to be *specifically* dependent upon the degree of the extinction of a CR. In the writer's study (**93**) an extra stimulus increased or decreased the value of a partially extinguished CR, depending upon the degree of extinction, while according to

Wenger its effect should be constant and at any rate not qualitatively different. Fourth, it has been found both that recovery from secondary extinction is much faster than that from primary extinction, and that extinction itself is much quicker in delayed, trace, transfer, and higher order CRs than in the usual simultaneous and first order CRs, results which can hardly be due to differences in proprioceptive facilitation. Fifth, for a number of responses and organisms in which all *behavioral* attributes of Pavlovian extinction have fully manifested themselves assumptions of muscular tension *differentiæ* must be extremely forced and improbable. Thus in voluntary or instructional conditioning a light may be the conditioning stimulus of some movement and a bell its conditioned stimulus. If the light is repeatedly applied no significant response decrement occurs, but, if the bell is similarly given, regular extinction—and under appropriate conditions recovery, disinhibition, and secondary extinction—results. To posit here some muscular relaxation that attends the sounding of the bell but is absent when the light is flashed, is obviously unjustifiable. Furthermore, Mikhailoff (**78–80**) has shown that all the phenomena of extinction are present in the conditioning of crustaceans and cephalopods, animals in which the proprioceptive mechanisms that are supposedly at the core of changes in muscular tension are little, if at all developed. Finally, it must not be forgotten that Hudgins (**42**) found extinction in pupillary CRs that have no known proprioceptive fibers and that Harlow and Stagner (**34**) noted this extinction even in curarized animals where possible influences of the postural substrate are of course out of the question.

The difficulties with the motivation or rather de-motivation theory of extinction are to some extent of the same order as those of the preceding hypothesis. Its chief weakness is that experiments plainly reveal that extinction is a definite specific form of modification with definite specific characteristics and not just a general state of progressive quiescence produced by a progressive depletion of a drive or an incentive and restored by *any* other adequate drive or incentive. The

statement that, "When the substitutive motivation is sufficiently potent and persistent, the CR will remain on the same level indefinitely" (**29**, p. 159), cannot, as far as the writer is aware, be empirically defended. The weight of evidence is in fact unmistakably against it. Indeed, in Finch and Culler's own study (**29**) the substitutive incentive, the buzzer, only delayed but did not prevent the final extinction of the CR, and it is very doubtful whether this failure of prevention was due entirely to the weakening of the effect of the buzzer, or that a new substitutive incentive would have fully rehabilitated the conditioning. More specifically, the de-motivation theory fails to account for the following well-established extinction attributes: (1) secondary extinction; (2) permanent non-restorable (unless reconditioned) CR losses of progressive extinction; (3) special extinction sensitivity of delayed, trace, and transfer CRs; (4) enormously greater extinction-restoring effectiveness of conditioning stimuli than of extra stimuli; (5) decremental effects of extra stimuli (incentives in terms of this theory) in the first stages of extinction and the very close general connection between extinction stage and extinction recovery. The theory must also make strained assumptions of incentive in the explanation of the extinction of such reflexes as the pupillary and the reactions of the voluntary or instructional type, and it leaves altogether unexplained extinction in non-conditioned responses and in the verbal material of Peak and Deese (**87**) where motivation was kept constant.

The extinction theory of substitute response and redirected conditioning—which enters, in varying degrees and forms, in the hypotheses of Culler, Guthrie, Wendt, and the writer—involves apparently four constituent propositions. First, there is the assertion that in the course of extinction the organism may manifest a number of reactions and activities that are not overt or active in conditioning periods or even in control periods and that, consequently, these extra activities disrupt, or help to disrupt, the conditioning process. Second, it is maintained that not only do these extra activities by themselves exert an interfering effect, but that the conditioned

stimulus actually becomes conditioned to these activities, thus undoing further the original results of the conditioning. Third, it is contended that this conditioning of the conditioned stimulus does not necessarily have to be redirected to some *activity*, but may well be redirected to a *cessation* of an activity, or, in other words, in the course of extinction the conditioned stimulus may come to signal 'what not to do' or 'what to stop doing' rather than 'what to do' and 'what to continue doing.' Fourth, it is argued that these extra activities and redirected conditionings that accompany—or result from—extinction are not just a factor of extinction or even a general characteristic of the extinction of some response and in some organism, but that they are universal and *sine qua non* to any and all extinctive processes as such. Wendt has emphasized the first, second, and fourth propositions, Culler and Guthrie in their concepts of 'inhibitory conditioning' and 'inverse conditioning' have stressed the third proposition and would apparently also hold to the fourth, although, unlike Wendt, they have each offered auxiliary postulates for the *initial* reversal in extinction. The writer has advocated propositions one and two, is willing to grant three with certain limitations, but is opposed to four.

The writer's opposition to the view that redirected conditioning is the cause of all extinction is based upon the following considerations. First, if all extinction is redirected conditioning, we should expect the correlations between the rates of conditioning and the rates of extinction to be positive and high. Actually, however, these correlations have been mostly negative, positive correlations occurring as a rule only with human subjects and in responses in which disturbing extra-conditioning factors may well have been operative (statement 12). Second, in the study of Miller and Stevenson (**82**) in which extra behavior during extinction had been recorded the correlations between this behavior and extinction—or the speed decrement of the main reaction—were small or negligible. Third, a theory of redirected conditioning cannot explain extinction of, let us say, a pupillary CR in a curarized cat (**34**) or even of a chromatographic change in an intact cephalopod

(78), since there can be no substitute response in these cases. (The last two criticisms do not apply directly to the concept of inhibitory or inverse conditioning but only to that of conditioning redirected to some other overt response.) Fourth, redirected conditioning cannot readily account for spontaneous recovery, sub-zero extinction, and disinhibition.[6] Particularly it cannot account for the regularity and specificity of the behavior of these attributes of extinction. Finally, as already stated, redirected conditioning *alone* does not constitute a *full* theory of extinction, inasmuch as it must also contain an interpretation of the initial cause of the redirection, namely the mechanism whereby non-reinforcement results in that a perfectly well-established CR loses in competition with a response that had been insignificant in the course of the conditioning. We have seen that the de-motivation doctrine is inadequate in this respect.

Guthrie's and Winsor's conception that the initial extinction reversal is produced by a change of attitude or affectivity is equally unacceptable as a general scheme. Consider the following situation. A dog has been conditioned to secrete saliva in one case with powdered biscuit as the conditioning stimulus, in another case with dilute HCl as this stimulus, and in a third instance with pilocarpin as the active agent. As the conditioning stimulus is withheld the ensuing changes of attitudes or affectivity should presumably be different in each case: irritability perhaps in the first, relief in the second, and probably not much change with the drug that acts systemically (in drug conditioning the conditioned stimulus is usually the injection of a physiological solution). And yet reported extinction curves have been quite similar in all three conditions. (Other examples of extinction in which attitudinal changes could not well be effective are those of unconscious conditioning—pupillary, spinal, misrepresented tasks.) In short, although motivational attitudinal or affective changes and redirected conditioning are no doubt very important in

[6] Secondary extinction might possibly be interpreted as resulting from the generalization of the redirected conditioning, which interferes with the normal function of the secondary CR.

initiating and furthering the extinction of a large number of responses, especially those of a so-called adaptive character, yet they do not relate to *all* cases of extinction and thus cannot be regarded as general factors of this process.

The two remaining theories of extinction, those of Pavlov and of Beritoff, differ markedly from the theories previously discussed in that they are too general rather than too specific, and thus explain 'too much' rather than too little. The objections to Pavlov's hypotheses are not only well-known but obvious and need not be repeated here in detail. They emanate mostly from the contention that, on the ground of simplicity, we should be more sparing in making new assumptions of brain mechanisms and less facile in deriving generalized organismic or neural states from specific particularized laboratory set-ups. It is indeed difficult to conceive that, for instance, repeated non-reinforcements of a patellar or an iridic CR could *per se* induce such states as sleep, hypnosis, catalepsy, or neurosis, an assumption which a consistent adherence to Pavlov's views makes necessary. Still, the writer feels, Pavlov's suppositions cannot be rejected outright, as suggested by some writers, but must rather be held in abeyance pending further experimentation in conditioning, in neurophysiology, and in related fields. Finally, Beritoff's theory, while operating with known premises of neural action, is nevertheless very general in a number of its phases. Its chief weakness is Beritoff's conception of response fatigue and its failure to recognize the specificity of the extinction decrement. However, his concept of 'reverse conditioning' is unquestionably a most valuable and plausible one and supplies an important contribution to the synthetic working hypothesis of extinction which is to be presented now.

A Synthetic and Hierarchic Working Hypothesis of Extinction

The hypothesis of extinction to be expounded here is synthetic in the sense that it draws some item from nearly all theories of extinction, and is hierarchic in that it considers extinction a multiple process in which a number of factors

(the number depending upon the level of the response and the stage of the extinction) of varying degrees of generality and specificity interact with one another in hierarchic fashion. It is named a working hypothesis, since, being primarily behavioral, it leaves the determination of the ultimate neurophysiological theory of the several factors of extinction to further research. It does maintain, however, that we already possess sufficient data to warrant the formulation of a general scheme of the neuromotor modifications in extinction.[7] The several factors which, according to the present hypothesis, make up a complete extinction hierarchy are: *A. Extinction Response Decrement; B. Extinction S-R Decrement; C. Reverse Conditioning; D. Inverse Conditioning;*[8] *E. Redirected Conditioning; F. Attitudinal and Affective Extinctions and Conditionings.*

It is assumed, mostly on the basis of direct experimental evidence, that factors *A*, *B*, *C*, and *D* operate in the extinction of any response and in any organism, that factor *E* is of significance only in a class of responses and conditions, and that *so far* factor *F* has appeared to be of critical effectiveness only in CR extinctions of adult human subjects. Moreover, it is contended that factor *B* is always greater than *A* and that *A* and *B* which act simultaneously are prerequisites and must therefore temporally precede all other factors excepting *F*, that factor *C* usually precedes *D*, and that factor *E* may come in any time after factors *A* and *B*.

Factor A.—Extinction Response Decrement.—Despite the obvious fact that the extinction decrement affects primarily the response to the stimulus *specifically* experimented with, there is little doubt that there is also some *general* response decrement. The writer tabulated the amount of the conditioning (unconditioned) salivation during extinction periods

[7] The writer does not agree to the total separation of the behavioral from the neurophysiological approach in conditioning. The work of Prosser and Hunter (89), Hunter (52), Loucks (70, 71), Light and Gantt (69), Dusser de Barenne (25, 26), and others are clear examples of the wide significance of neural CR and extinction studies.

[8] The term 'reverse conditioning'—borrowed from Beritoff—differs from 'inverse conditioning'—taken from Culler—in that the former signifies return conditioning to a pre-conditioning state and the latter means conditioning in an opposite direction.

in 63 separate experiments from Pavlov's laboratory and found this salivation to be 13.4 ± 2.8 per cent less than the amount of the conditioning salivation in corresponding conditioned periods. Pavlov himself partly recognized this phenomenon when he stated: "Sometimes secondary extinction reaches a profound degree, involving even the unconditioned reflexes" (**85**, p. 54). In other Russian laboratories—Bekhterev's, Beritoff's, Ivanov-Smolensky's, Krasnogorski's, Zelyony's—reduction of the conditioning response during extinction have similarly been noted, and in a related field of adaptation to the GSR Coombs (**16**, p. 267) concludes that ". . . galvanic adaptation to auditory stimuli is in part general and in part specific to the stimulus." An affirmation of a general extinction response decrement is thus in line with existing evidence. It has the advantage both of obviating, to some extent, the need of explaining secondary extinction by 'irradiation and concentration of inhibition' and of better accounting for the qualitative distinctness and lability of secondary as compared with primary extinction. That response decrement does not, however, take care of all extinction, nor even of all secondary extinction will become evident presently.

Factor B.—Extinction S-R Decrement.—Besides the response decrement there must also occur as a result of extinction another diminution the locus of which can by no means be considered the effector mechanism itself. This is clear not only from the the fact of a much greater CR decrement to the extinction stimulus but also from the fact that the decrements which occur when the CR is evoked by other stimuli are apparently in some way directly proportional to the similarities of the other stimuli to the extinction stimulus. A pre-effector decrement that somehow generalizes or transfers has to be assumed, although this decrement—as well as the effector decrement—need not be of the nature of an inhibition nor its transfer an irradiation of this inhibition.

While a conclusive account of the decrements and their transfers must await the continuation of the pioneer studies of Prosser and Hunter (**89**) and Hunter (**52**), the writer feels

that his theory of asynchronization offers a tentative explanation of its mechanism. According to this theory, in extinction and negative adaptation, in contrast with fatigue, the functional state of each neural and effector unit is actually improved through the repeated stimulation, but this improvement results in a wider asynchronization of action between the individual units and consequently in an impairment of the response. Transfer would then be due to asynchronization in common units and the smaller effector decrement would be due to the smaller number of units in the effector mechanism.

Another hypothesis, which is not altogether unrelated to that of the writer, is the assumption of chronaxie changes, an assumption that gains fresh support from recent experiments. In a series of studies Drabovitch and his associates (**24**) have gathered evidence that shows conditioning to be accompanied by greater isochronism between cortical, subcortical, and peripheral chronaxies. Extinction may accordingly be postulated as a progressive heterochronism between these chronaxies. In this connection the experiment of Magnitzky (**72**) who compared a nerve-muscle preparation by means of chronaxie changes in fatigue and in Wedensky inhibition is of high suggestive interest, although it does not of course deal with extinction proper. Magnitzky found that during fatigue the rheobase of the nerve decreased to 51 per cent of its normal value, and the rheobase of the muscle increased to 378 per cent, while during Wedensky inhibition reverse changes occurred, the nerve rising to 378 per cent and the muscle dropping to 58 per cent of their usual rheobase readings. However, whatever the neural basis of these decrements may be, the point to be emphasized here is that *this 'decrement by repetition' is a fundamental ultimate form of behavioral modifiability that no doubt underlies many other types of behavior changes rather than emanates from them.*

Factor C.—Reverse Conditioning.—The concept of reverse conditioning embraces two propositions. First, that during conditioning some connections are formed between the conditioning stimulus and the original response of the stimulus-to-

be-conditioned, or conditioning center→original center of conditioned stimulus. Second, that during extinction, when the main connections between the conditioned stimulus and the conditioning response are weakened, the reverse connections gain the upper hand, heighten the efficacy of the original center of the conditioned stimulus, restore the original response to the conditioned, and thus further greatly the deconditioning or extinction process. Direct evidence on these propositions is not common, since in the usual CR experiments the original response of the conditioned stimulus is small, often only implicit, and seldom measured. However, when such measurements have been made, reverse conditioning and restorations of original responses were clearly apparent, as in the studies of Narbutovich and Podkopaev and of Bernstein (**82**, **11**; conditioning both ways) and in that of Hilgard and Campbell (**36**, restoration of original response in extinction). Indirect confirmation of this conception is obtainable in the work of Beritoff (**10**) and occasional observations of this sort have not been unknown in Pavlov's laboratory. Reverse conditioning also accounts, in the writer's opinion, for some of the failures of American investigations in which the conditioning had been very much massed, since short intervals between individual CR trials favor reverse connections between the conditioning stimuli of one trial and the subsequent conditioned stimulus of another trial. Similarly, reverse conditioning may be thought of as *one* of the causes of the difficulty of backward conditioning. Finally, it should be restated that reverse conditioning is conceived here merely as a contributing factor of extinction that becomes active only after some decrement by repetition has already been effected.

Factor D.—Inverse Conditioning.—In the writer's experiments with the extinction of salivary CRs in human subjects the conditioned stimuli produced at times less salivation than their normal controls. In Garvey's study (**10**) of adaptation to respiratory changes subnormal values were similarly noted, while Culler's concept that "the bell becomes a safety-signal instead of a danger-signal" and even Guthrie's 'inhibitory

conditioning' are not unrelated to the writer's findings. All these phenomena may be subsumed under Culler's 'inverse conditioning' by stating that, while in the process of conditioning the conditioned stimulus comes to signal an appearance or an increase of some response, in extinction the same conditioned stimulus signals a decrease, cessation, or inversion of the response. Our understanding of the mechanism of this change is facilitated by the knowledge that CRs to cessations of actions have been reported (**125**) and by the plausibility of assuming that a stimulus in one extinction trial may actually become conditioned to the response decrement of the succeeding trial. More specifically, the last assumption means that, if a stimulus S in trial T produces response R and in trial $T + 1$ response $\frac{R - 1}{R}$, then the stimulus actually becomes connected in some way with the act of effecting $-\frac{1}{R}$, or the response decrement. Further, it should be mentioned that, like reverse conditioning, inverse conditioning is by itself an inadequate hypothesis of extinction and needs the supplementary concept of decrement by repetition to explain the *initial* extinction reversal. It is also conceivable that inverse conditioning becomes overtly significant only in the later stages of extinction when the conditioned incremental tendencies of the conditioned stimulus have gradually become offset by the extinction decremental tendencies of the stimulus and that thus inverse conditioning follows temporally reverse conditioning. Experiments substantiate this supposition (**91**).

Factor E.—Redirected Conditioning.—The four factors of extinction so far discussed have dealt only with probable changes resulting from the actions and interactions of only *two* single reaction patterns, the conditioning and the conditioned responses. The factors and the changes could indeed be postulated as well for a hypothetical organism that is capable of only two reactions. There is little doubt, however, that in ordinary CR studies other reactions *may* materially influence the extinction process. The assumption that during extinction a number of reactions that had been held in abey-

ance during conditioning are released, that these reactions advance the extinction, and that the extinction stimulus becomes conditioned to them, is fully justified and is subsumed here under 'interference and redirected conditioning.' Nevertheless, it should be realized that the effectiveness of this factor will vary greatly from one CR situation to another. It may be very significant when a monkey, an infant, or even a free-moving rat is deprived of its customary food, but is probably of little if any importance in the pupillary or wink alterations of a dog or a cat, the color changes of a mollusc, or even in the defense reactions of a harness-habituated sheep or dog and in the salivation of a well paid human subject who had been derpived only of a piece of chewing gum. In general, our present knowledge of the role of these extra activities in extinction is extremely limited, and simple parsimony forbids us assigning too great an explanatory value to them when other principles are available.

Factor F.—Attitudinal and Affective Extinctions and Conditionings.—In the experiment of Miller and Cole (**80**) knowledge of results affected considerably the course of extinction of a conditioned wink response of human subjects. In the writer's series of studies the extinction differences between subjects who had been aware and who had not been aware of attempts to condition them were even much more pronounced. Other and more specific evidence of this effect has not been obtained, and in the case of animals existing methodologies practically preclude any investigation of these central factors in conditioning and extinction. It is certainly conceivable that the wide differences between the CR and extinction results of Schlosberg (**97, 98**) and of Skinner (**101–104**) in rats are attributable to the varying degrees of participation of the animals' total activity stream in the conditioning process. Indeed, it may not be too far-fetched to posit here that differences between conditionings and rewards and between extinctions and non-rewards, are primarily functions of differences between the extent and the coordination of CRs and CR systems in restrained and in free-moving animals.

However, more research must come forward before anything more definite can be advanced in this direction.

Miscellanea

Failure to secure extinction.—With the exception of the results of Hilgard and Marquis (36) with the wink response in four monkeys, the cases in which no extinction was secured fall into three main classes: consciously controllable responses in adult human subjects, very simple low-level reactions (pupillary, decorticated and curarized animals), and the very special case of conditioned verbal stimuli. The first class may be accounted for, as Razran has shown, in terms of interfering subjective attitudes, as a subject who *remembers* a connection between a stimulus and a consciously controllable response and who has formed a definite attitude about the connection may obviously reinstate it or prevent its extinction. Of the second class, the easiest to explain is the non-extinction of conditioned jumping reactions in the curare experiment of Harlow and Stagner (34), since under their set-up the animals were also unable to condition to this type of a reaction. On the other hand, the extinction difficulties in the decorticated dogs of Baschmurin (5) and in the decorticated pigeons of Popov (88) are plainly contradicted by the greater number of findings (8, 31, 66) in which decortication did not at all impede the extinction. Equally unexplained must remain the results of Baker (4) with pupillary CRs to subliminal stimuli and those of Menzies (16) with vasomotor CRs to some stimuli, as the possible assumption that extinction requires a higher level of neural organization than conditioning does not appear to be justified. Finally, Hudgins' discovery of a differentia in the extinction of conditioned verbal and conditioned sensory stimuli is of course highly significant, inasmuch as it suggests the possibility of a vicarious CR functioning of the verbal or symbolic mechanism, but this finding, too, must await further confirmation.

Extinction of verbal material.—Peak and Deese's (87) results of extinction of verbal material require a supplementary clarification of the writer's view that "consciously controllable connections may become independent of the 'energy of' or 'reinforcement by' the original stimulus which caused the connection to be formed." The emphasis is here upon *controllability* rather than *consciousness* and upon *may become* rather than *do become*. In other words, the writer's argument is one of common sense that there can be no extinction when there still is a conscious association. No matter how many times the metronome may be presented without the pretzel, the subject may *reinstate* the CR to the metronome if he *remembers* the connection between the two. In the experiment of Peak and Deese the memory itself was the subject of the investigation and a memory extinction process may well have been found. Indeed, in the writer's recent study, in which a large number of stimuli were asso-

ciated with food and another large number without food and the subjects did not remember the particular associations, an extinction phenomenon was similarly noted.

Extinction and dominance.—Space does not permit any detailed discussion of the relation of the account of extinction presented here to the writer's theory of dominance. It may be mentioned, however, that this theory has been very much confused by a number of writers, that it is primarily a behavioral rather than a neural hypothesis, and that it is by no means synonymous with drainage. It should be further stated that the present extinction account does illustrate 'dominance' at several levels of organismic behavior: asynchronization, the dominance between component units of a reaction pattern; inverse conditioning, the dominance changes in the direction of a reaction; reverse conditioning, the dominance shifts between the conditioning and the conditioned reaction; and redirected conditioning, the dominance alterations in competing reaction systems.

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[MS. received December 6, 1938]

A NOTE CONCERNING McCULLOCH'S DISCUSSION OF DISCRIMINATION HABITS

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In *Comment on the Formation of Discrimination Habits* (5), which appeared in the PSYCHOLOGICAL REVIEW, Dr. McCulloch attempted to explain the apparent inconsistency in experimental evidence relating to the so-called 'continuity' and 'non-continuity' theories of learning. Krechevsky (4) pointed out that continuity, or the lack of it, is critical in deciding between two types of learning theory. "One of the corollaries of the 'non-continuity' assumption would be that if the significance of the stimuli are reversed before the animal begins to 'pay attention to' them (*i.e.*, during the pre-solution period) it should not necessarily make for any slower learning of the reversed problem. . . . On the other hand, assuming the 'continuity' assumption, we would definitely expect just the opposite set of results" (4, p. 112–3). His experimental data show that such a reversal during the 'pre-solution period' need not inhibit final learning. McCulloch and Pratt (6), on the other hand, found (as, indeed, did Krechevsky) cases where it did. McCulloch, in his more recent article, attempts to reconcile this difference and to explain Krechevsky's results in terms of his own theory of discrimination learning. In this the present author feels that he fails in several instances; indeed, it seems likely that the continuity of the McCulloch and Pratt experiments falls more readily into Krechevsky's explanation.

In the first place there are certain intrinsic characteristics of the string-pulling technique which are peculiar to it. One of the most important of them is the fact of a forced adjustment to the weight differential (the 'relevant discriminanda'

in the McCulloch and Pratt experiment). This requires the animal to react differentially to the finally 'relevant' discriminanda during the so-called 'pre-solution' period. This takes place regardless of whether or not his hypothesis—defined in other terms—is directed toward this differential at the time. It has been shown (**2**) that a forced adjustment to certain factors in the experimental situation has a striking effect on the learning itself. It may be that this peculiarity of McCulloch and Pratt's technique is sufficient to render the situation atypical and to require an especial explanation. Some other peculiarities of the string-pulling technique are also relevant here.

In attempting to facilitate learning in some unpublished experiments employing the string-pulling set-up, the author had occasion to increase the weight differential from an opposition of 50–150 gms to about 50–700 gms. Previous to this almost all the animals showed spatial hypotheses (*i.e.*, choices of left or right to a percentage at least 3 sigma units from chance). On the day of this change almost all the animals immediately showed equally significant 'lighter' choices. This 'lighter correct' hypothesis appeared, in most cases, fully developed, showing no preliminary period in which choices went gradually from chance to 80–90 per cent lighter. In view of this and of the fact that it seems reasonable to expect a preference for the lighter, how might this affect the McCulloch and Pratt string-pulling experiment? It seems possible that if, instead of having had preliminary training on lighter positive and reversal to heavier positive, the opposite procedure had been used, the retardation might not have appeared. Such a preference is an hypothesis in a very real sense, though without strict operational definition. Let us assume that for the first 28 trials (the amount of training before reversal for McCulloch and Pratt's Group II) the animals were largely concerned with their orientation in the apparatus and were not *psychologically* oriented in the direction of the discrimination. However, this preference-hypothesis is present, *though not shown clearly in the error scores* before the introduction of the heavier weight. Further,

Krechevsky (4) says that the 'residual neurological effect' of a 'rewarded' response or a 'punished' response will apply only to those stimuli to which the animal is attending. If this preference for the lighter is present in this case it will serve the same function for the non-continuity theory as any other hypothesis and reward or punishment will enhance or discourage its continuance similarly. In terms of this analysis the McCulloch and Pratt Group II, which has a shift from lighter positive to heavier positive, not only presents no difficulty for the non-continuity theory, but would be predicted by it. Presumably reward on this basis would act to further its continued existence, since one of the characteristics of an hypothesis is purposiveness (9). This, however, would not be the case in other set-ups where no such preference exists (*e.g.*, the jumping stand). It seems possible, therefore, that the negative transfer in Group II was an artifact of the experimental method. Much of this depends on the question of whether or not we may speak of an hypothesis existing when the error scores do not yield the operational criterion of choices 3 sigma units from chance.

In regard to this point a misconception of the nature of an hypothesis seems to have arisen. *Hypothesis* is defined in terms of certain systematic behavior patterns. It seems clear that too strict operationism clouds the real issue. The display of behavior which is 3 sigma units from chance in terms of choices seems, conceptually, not to be an hypothesis, but the application of an hypothesis—and there may be real differences in the implications for experiment. Indeed, if we are to interpret the final display of such systematic behavior, we must assume that the hypothesis precedes it. An organization of the situation which enhances a particular factor (or, in Spence's terms, a certain imbalance between 'excitatory' and 'inhibitory' tendencies) is logically prior to and necessary for such behavior. In the McCulloch and Pratt experiment Group III was transferred when they "seemed to begin to discriminate" as measured by a drop in the error curve, on the assumption that they would then be in 'a labile state and most docile' (6, p. 274, 278). This assumption, however,

is not true. One would expect them to be very little labile at this point, after their *reorganization*. The labile period of ambivalence of items must precede the behavioral evidence of 'insight' or 'hypothesis.' If this is the case, Group III (McCulloch and Pratt) presents no difficulty for non-continuity theory. Krechevsky's Group III—which *did* show negative transfer, or so-called continuous learning, and on which McCulloch expends some little discussion—may be an example of the same thing. Since Group II (Krechevsky) did *not* show negative transfer with only 20 trials difference, the change was occurring rapidly at this point. It seems possible that Group III (Krechevsky) had the state or organization which produces systematic behavior, but had not yet overtly demonstrated it.

Moreover, *multiple* hypotheses appear more frequently in the string-pulling technique than, for example, in the jumping stand. For instance, an animal would show a slight preference for, say, light and left. If heavy appeared on the right he would not take it, but he sometimes would on the left. Similarly, if light were on the right he sometimes would take it, and almost always would on the left. The conflict of these two hypotheses, plus a few choices which were not systematic in any easily discoverable direction, served to bring the animal's score down very near to chance. Only a very close scrutiny of both behavior and scores would show the systematic character of the behavior. Krechevsky (**3, 4**) suggests that these multiple hypotheses may be at the base of the apparent continuity, and in view of their special role in the string-pulling technique it seems worth while to consider the possibility in the McCulloch and Pratt experiment.

McCulloch (**5**, p. 76) discusses which stimuli do and which do not affect the sensorium and bring about a reinforcement. "All stimuli acting at the time of response become associated with that response." Later, however, he says, "Increments and decrements in response tendency should occur from the very first only if the relevant stimuli so affect the sensorium that the associations are similar to those upon which the final habit is based." What does this mean in terms of the

increments and decrements in a given situation? It seems that as long as stimuli are affecting the sense organs there can be no question of orientation on associationistic assumptions, and excitatory tendencies must go right on being built up. 'Attending' in the sense in which Spence (8) speaks of it (*i.e.*, adjustments for sensory reception) does not seem to fit Krechevsky's experiment, since any 'orientation of the head and eyes' to the stimulus complex which allowed the animal to react to spatial position, or brightness, for instance, would—as far as simple optics are concerned—similarly facilitate the perception of the relevant discriminanda. Unless this orientation is a *psychological orientation*, it seems to have no bearing on the experiment; if it is a psychological orientation, it seems to be identical with Krechevsky's formulation.[1] The only orientation which seems capable of explaining the situation is one arising out of the dynamics of the situation—*i.e.*, the organization of the learning situation. That is to say, the organization of the factors involved in the learning situation is such as to enhance a particular one, with respect to which the animal displays this 'psychological orientation' and subsequent systematic behavior.

How can this 'psychological orientation' be stated experimentally? Lashley (7) found that in visual stimuli containing two sets of possible discrimination in a figure ground relationship (*e.g.*, a square on horizontally striped ground versus a triangle on vertically striped ground), one will be dominant; which one will be dominant, Lashley reports, depends upon previous training. If an animal were set, without previous training, to master this discrimination, he would, presumably, solve the problem with one of the two sets

[1] It may be worth while to point out here that the recent attempt of Gulliksen and Wolfle (1) to explain these results runs into the same difficulty. They say, "It may be assumed that the early trials constitute a period of adaptation to the experimental situation and that learning does not begin until some time after training has started" (1, p. 249). If this is so, it is hard to understand the role of 'c' and 'k' in their formulæ, where they are treated as constant increments regardless of their position in the learning process. If, however, they deny this constant character of the increments—which they have explicitly stated in their mathematical formulations—their explanation in terms of 'a period of adaptation' sounds very much like an hypothesis or set of them, based upon factors which are not relevant to the final discrimination.

of discriminanda. That is to say, his hypothesis would be, for instance, in terms of the square as positive versus the triangle as negative. In this case, according to the non-continuity assumptions, the repeated reward of the horizontal stripes and the punishment of the vertical stripes, which follows necessarily upon the reward and punishment of the square and triangle, would not necessarily facilitate subsequent learning of the stripes by themselves, since the animal was not attending to them in the discrimination. On the continuity assumptions of McCulloch and Spence, however, such differential reinforcement must necessarily be equally effective, since the orientation in Spence's terms (*cf. supra*) is present for both sets of stimuli. McCulloch says, "All stimuli acting at the time of response become associated with that response" (**5**, p. 76). If this is so, then the experiment above seems to be a critical case for the two theories. If such orientation—which is clearly a psychological orientation, arising as a response to a particular organization of the visual field—is effective in determining the role of the changes brought about by reward and punishment, then the continuity hypothesis would be shown to be inadequate to explain the situation.

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[MS. received February 20, 1939]

REPLY TO A NOTE ON DISCRIMINATION HABITS

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With reluctance to use journal space in this manner, I shall attempt to clarify from my position some points raised by Mr. Haire in his paper (1) in the current number of this journal. Discussion will be restricted to three topics, namely, the interpretation of certain experimental results, the intended meaning of some statements previously made by me, and the importance of design in crucial experiments.

It was found by McCulloch and Pratt (5) that negative transfer after reversal of cues was proportional to training before reversal regardless of whether the animals had shown no indication of learning (group II), had just shown such indication, or had been long trained beyond mastery. Nevertheless, Haire contends that the results with group II may have been an artifact of method. Let us examine the basis for the claim that negative transfer was due to the use of the lighter weight as positive cue before reversal, and that the transfer would not have occurred had the heavier been positive.

According to Haire, negative transfer after reversal was due to the fact that a resistance preference, not manifest in performance, was rewarded before reversal.[1] Let us suppose that the rats actually did prefer the lighter weight and that the "continuance" of this preference was "enhanced," to use Haire's terms, by food-reward. This enhanced continuance would then result in negative transfer after reversal of cues.

[1] Krechevsky also has suggested that the experiment was 'loaded' in favor of a possible negative transfer effect. "In such a situation, assuming that the animals already had begun to react to the weight differentials, we might expect more negative transfer than would have been the case had no preference differences been involved" (2, p. 131).

Suppose, however, that the opposite stimuli-reward relations had held (heavier + before reversal and lighter + after reversal). And let us assume, again, that the lighter weight was preferred. Differential reward before reversal, in such case, to follow Haire's analysis, should "discourage" the "continuance" of preference for the lighter weight. It follows that discouragement of the continuance of preference for the lighter should result in negative transfer after reversal. In other words, accepting Haire's assumptions regarding reward and punishment of a preference, *negative transfer should result after reversal, regardless of whether reversal is made from lighter to heavier or vice versa.* It seems, moreover, that Haire's argument assumes the applicability of the cumulative hypothesis, namely, that before reversal associations were established with relevant cues, even while the animals were exhibiting consistent position responses.[2, 3]

A misunderstanding apparently has arisen from lack of clarity in my previous discussion. Haire selects two statements, namely, ". . . all stimuli acting at the time of response become associated with that response" and ". . . *increments and decrements in response tendency should occur from the very first only if the relevant stimuli so affect the sensorium that the*

[2] Although the *mere existence* of resistance preference does not, according to this analysis, produce negative transfer, it may be noted that the fact which Haire mentions, that rats show preference for the lighter when the differences in weights are sufficiently great, had already been demonstrated when the results of McCulloch and Pratt were reported; furthermore, it had been shown in the same study (3) that with the same weights used with group II, rats failed after extensive training to show consistent preference for the lighter. It should also be pointed out that if weight discrimination is atypical because of resistance preference, as Haire suggests, various other discrimination situations also are atypical, as is illustrated by the fact that the amount of training for brightness discrimination in normal rats may depend upon whether the brighter or darker stimulus is positive, and by the fact that primates frequently require more training if one, rather than the other, of two visual cues is positive.

[3] It is frankly noted that the negative transfer of group II might have been artifactual for other reasons than those which Haire suggests. This would have been the case if, before reversal, there had been an *increase* in preference for the lighter *as a function of something other than food-reward.* The factors, ranging from climatic effects to systemic adjustment to the resistances, which might conceivably have produced an increase (or decrease) in preference for the lighter weight, are indefinite in number. Suffice it to say that, although in my opinion the evidence is presumptive that these factors were insignificant, they were not ruled out by the experiment.

associations formed are similar to those upon which the final habit is based" (4, p. 76, 78). And seemingly in criticism of these statements he says, "It seems that as long as stimuli are affecting the sense organs there can be no question of orientation on associationistic assumptions, and excitatory tendencies must go right on being built up." As my statements were intended, an exactly opposite inference should be drawn, namely, that orientation is extremely important. The first statement is simply one of generally accepted association theory. The second states that associations formed in early and late training are "additive" only if similar. They are similar only if the *effective stimulations* are similar. Thus, with training a stimulus configuration (S) becomes associated with a response (R). If at first R occurs while, because of different orientation, S_1 (but not S) is effective, a connection between S_1 and R (but none between S and R) is made. Only when S and S_1 are sufficiently similar (as determined by similar orientation) will early training help to establish the connection that is finally made.

In the last paragraph of his note, Haire outlines an experiment designed to be crucial for association theory versus a "non-continuity" theory. In my opinion, the experiment is indeterminate as far as the interpretation that discrimination learning occurs in a cumulative manner is concerned. If specific results can be deduced from the cumulative hypothesis because the precise orientation assumed, and hence the effective stimulation, is unknown and perhaps cannot be determined. It is possible that, with both sets of stimulus patterns present, an orientation might be made which would allow differentiation of the members of only one set of stimuli to be made. For instance, with both sets present, the rats might fixate upon a certain patch which would be black if the card were positive and white if it were negative. Yet with the forms alone present this patch might be white for both cards, and as long as this fixation were maintained, the forms might be indiscriminable.

Lest it be thought that the cumulative hypothesis is not subject to disproof, and hence not scientific, a suggestion will

be made for an experiment, one result of which would definitely disprove the hypothesis. This experiment resembles that proposed by Haire, but avoids ambiguity regarding orientation. Rats would be trained, with a pull-in technique, concomitantly with visual (*e.g.*, brighter +) and weight (*e.g.*, heavier +) cues, with the differences in difficulty so arranged that the visual discrimination would be learned considerably more rapidly than the weight discrimination. Thus *the conditions of the experiment would assure differential stimulation by the auxiliary, weight, cues.* After training to mastery on visual cues, further training to mastery with weight alone should be less than that of a control group which had not had the previous training. If this last statement proves incorrect, the cumulative hypothesis will be definitely disproved. It should be noted, however, that *any* result of this experiment, or of that proposed by Haire, would be in harmony with the non-continuity theory. For, depending upon the results, it could be said that with both sets of stimuli present the animals (a) were attending to one set of stimuli, or (b) were attending to (had non-operationally-defined multiple hypotheses for) both sets of stimuli. In fact, I am at a loss to think of any results which might not be reconciled with the non-continuity theory.

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